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EDITORIAL

By the time this Special Number of the *British Journal of Delinquency* is in the hands of our readers, the Third International Congress on Criminology will be in full session at Bedford College, London. The occasion is in every respect a notable one, and the Editorial Board felt that it might be appropriately signalized by the issue of a Special Congress Number devoted in the main to a problem towards the solution of which the energies of Congress members will be almost exclusively bent. However various may be the views on Recidivism, there can be no question that the problem lies at the heart of all criminological endeavour. As, however, the issue is now *sub judice*, the Editors have decided to refrain from exercising their roving commission of editorial commentary until such time as the deliberations of the Congress have reached their final expression. The same consideration applies in the case of the companion problem of Prediction, which is without question one of the most promising scientific developments in the study and prevention of crime, and at the same time one of the most controversial issues that has appeared on the criminological horizon in recent times, as indeed may be gathered from some of the contributions to this special number. By a happy chance the subject was submitted to preliminary discussion at a meeting of the Scientific Group for the Discussion of Delinquency Problems, held in London in April last, and a full report of this symposium is published in the present issue.

The Board wishes to take this opportunity of welcoming with cordiality those members of the International Society who have come from abroad to take part in the deliberations of the Congress, and to express the hope that their visit will be of happy augury for the future of the Society.

SYMPOSIUM ON PREDICTIVE METHODS IN THE TREATMENT OF DELINQUENCY ¹

I. LESLIE T. WILKINS (LONDON) ²

It is, I understand, my duty this evening to put forward the case for prediction methods in penology. With the notice of this meeting members will have received some notes on the work recently completed by Dr. Mannheim and myself in applying prediction methods to Borstal boys (see *Appendix*). These notes show what the method can do with restricted data in a narrow field. I do not think that anyone can argue that prediction methods do not work. Those who object to prediction methods do not usually base their case on the grounds that they are ineffective, but on grounds related to the fundamental concepts upon which prediction systems are based. If I were acting as counsel for the defence of prediction methods in penology, I should, I think, be content merely to rest my claim on what they can do.

I should ask for proofs that any other methods could equally do their job. I should, of course, require that any evidence put forward was as rigorously based as are the proofs of the prediction method itself. I shall, however, this evening (because this is a scientific group) not rest my claim solely on the case that the proof of the pudding is in the eating—I shall tell you something about how the pudding is made!

Prediction methods can be constructed within a completely closed framework of three matrices: (1) A matrix of available information (*Information*); (2) a matrix of objectives, or objective desired (*Control*); (3) a matrix of possible decisions (*Decisions*).

No causal hypothesis is necessary—we may go straight from *information* to derive a maximal system of *control* with respect to the end *objective*, via the possible *decisions*. There seems no point in asking for 'best estimators' or 'best explanations' without reference to the concept of 'best for what'.

¹ Being the report of a Symposium held under the auspices of the Scientific Group for the Discussion of Delinquency Problems at the Medical Society of London on 26th April, 1955.

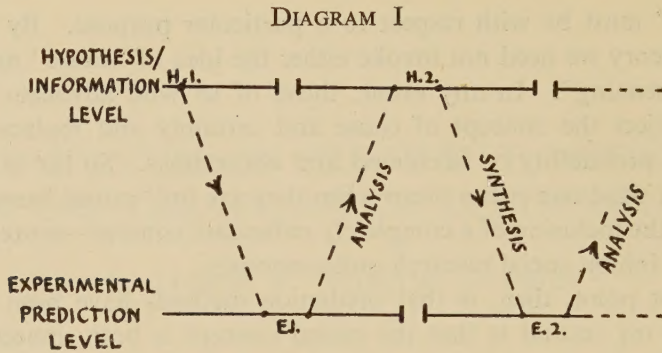
² Although I am correctly described as a research officer of the Government Social Survey, the views I express are my own and are not necessarily shared by any of my colleagues. The Government Social Survey is not to be confused with Social Surveys Limited, which is a private company.

Our 'best' must be with respect to a particular purpose. By the use of decision theory we need not invoke either the idea of 'cause' nor need we discuss 'meaning'. In any event, those of us who advocate prediction methods reject the concept of cause and certainty and replace it with a concept of probability or likelihood and uncertainty. So far as I am able to interpret what our critics mean when they ask for 'causal bases,' they are asking for the inclusion of a completely redundant concept—more, a concept that might inhibit social research quite seriously.

My first point, then, is that prediction methods have been proved to work, and my second is that the causal concept is both unnecessary and invalid.

My third point relates to experimentation. Let me say at once that in such fields as may make direct use of experimental methods (to rigorous and efficient designs) prediction methods in the particular form I have just discussed might not be necessary—other models equally based on the likelihood concept but of more potential power should be used. By direct experimentation I have particularly in mind designs where the variables are uncorrelated. But such direct experimentation in the social field is generally impossible. The prediction method, as I have outlined it, makes possible a form of 'quasi-experimental' design (I do not like the term, but some distinction seems necessary) where instead of the scientist himself making changes in his variables, he notices and measures what the administrator is doing in the ordinary course of his decision making. Without interfering with the administrative machine, prediction methods make it possible to derive a (quasi) experimental set-up through utilizing decision theory. An example of this method is given as Table I of the Appendix. Here we show that treatment in open Borstals is more likely to result in success than treatment in closed. We could have derived much the same result by (proper) experimental methods; for example, by sending boys at random to the two forms of treatment. But should we have been allowed to do this? If so, this process might have given us a simpler proof and a more direct one, but in fact it would have contained less information. The predictive method provides a procedure for experimentation where experimentation is otherwise impossible. If then we wish to experiment in the social field, in the great majority of cases we must resort to prediction methods. If, however, you reject experimentation, I would submit that you are rejecting the scientific method. If you accept experimental methods, you can either do nothing, or accept prediction systems.

The predictive method is, as some of you will have realized already, closely related to the hypothetico-deductive method. I think I can best explain the sort of way in which I visualize scientific progress by a diagram.



(With acknowledgement to G. E. P. Box and apologies for some extending of his ideas.)

Let us suppose that we start at H_1 , that is with a first set of hypotheses, whether implicit or explicit. We shall find that we are not able to get very far, our deductive reasoning will lead us to conflicting statements—an 'iron curtain' which we cannot get past by the use of reasoning or theory alone; we must by-pass this 'block' by experimentation. Then when we analyse experiment E_1 , we shall out of this analysis derive further hypotheses (set H_2); the analysis leads to further hypotheses and the hypotheses by synthesis to experimentation. We move slowly, one stage at a time and cycle by cycle. Leaps may sometimes occur, but we must not expect these, but should work as though they might never occur. By this method we keep pushing the frontiers of our ignorance further back. But not back to *causes*. We must always be empirical—always improving our ability to control. Indeed, as I see it, the measure of our understanding is the measure of our ability to control. We are not otherwise concerned with understanding. The understanding of the scientist (in the usual meaning of that word) must depend upon the scientist's ability to understand. When we find one solution more understandable than another, we may feel happier, but happiness is not science, nor is it any criterion of scientific validity. We must have surer ground for a measure of our knowledge than merely that we find one solution more understandable or more plausible and others less. Plausibility may be merely a blinding factor—it may be very dangerous indeed. In the Borstal study there was ample evidence of the danger of 'face-validities'. The plausible solution (if ever any solution could be more plausible than another to a scientist) needs testing, perhaps even more thoroughly by empirical methods than one which does not so commend itself. The scientist must watch always for self-deception, he must never be satisfied with the criterion of intuitive happiness, nor by the fact that a result fits a theory—it may equally well fit a large number of theories—but by the evidence of prediction and control and by this evidence alone.

In conclusion and summary then, I have claimed that (i) prediction methods have been shown to work. The proofs of the predictive system as being operationally effective are completely rigorous and reproducible; (ii) the concept of 'cause' is not useful—it is to be replaced by a concept of probability. We may obtain control without 'cause' or its related concepts; (iii) what looks like a plausible solution or explanation is no criterion—the only criterion is our ability to predict and control.

If you are still not satisfied with the predictive method, may I ask you to introspect and quite honestly answer two questions: 'What do I want from research?' and: 'Why do I want it?' A reply like, 'I want to understand delinquency because I like to understand things,' is not the scientist's final reply. If you say: 'I want to understand because I want to be able to help delinquents,' then you are really saying that you want to be able to exert a control. Then, I think it follows that you will agree with me! Prediction methods are a tool which will most directly and certainly enable this position of control to be reached. How would I start? Very simply, you tell me: (i) what you want to control; (ii) how you want to control it; and (iii) let me have access to what information you have or can find which might possibly be relevant. We can then start with our first set of equations. We shall most likely get little from our first solution, but we may clear away a lot of dead wood, and we should be able to proceed step by step from there onwards. We shall not try to get to the end of the road ('causes'?) in one single jump, but shall make our moves systematically from stage to stage.

APPENDIX

SOME NOTES BASED ON 'PREDICTION METHODS IN RELATION TO BORSTAL TRAINING', BY
MANNHEIM, H. & WILKINS, L. T.

I. Prediction methods are based on 'decision theory' concepts. The results are related towards 'action' rather than specifically to an 'increase in knowledge'. Single problems are isolated and dealt with singly—we do not seek any general solutions. Prediction methods are possible within a closed framework of (a) a body of information (given); (b) a limited number of actions are regarded as 'possible'; (c) an objective is clearly defined (e.g., minimum loss).

Given this framework we may see how best to act (b), in the light of the information (a) so that we have the best chance of obtaining (c). Our solutions are 'best' linear solutions, but more complex models would be possible within the same framework. There is no need for an integrated theory—the information matrix may be derived from many or conflicting theories.

II. Four principles are involved: (a) repeatability/reproducibility; (b) validity and power (related to objective); (c) efficiency (both to reduce labour and for (b); (d) simplicity (to reduce error in application, etc.).

III. Prediction methods as applied to date lead to a critical attitude towards the concept of 'causes', and the related concepts of 'understanding' and 'meaningfulness'. At the same time the methods provide a way of avoiding these issues and deriving useful results without them. Subjective judgments have also shown up very badly. No subjectively based item has so far been found to contribute anything additional to the predictive value obtained by the efficient use of 'factual data'.

IV. The prediction method may be used to develop a 'quasi-experimental' design, so that use may be made of variations in administrative action *as though* such variations were part of an experiment. This method has been used in the Borstal project to test differences between 'open' and 'closed' institutional treatment.

EXAMPLES OF SOME RESULTS FROM THE BORSTAL STUDY

Use of Prediction Results to Test Training in Two Types of Institution

Risk class	Success Rate (Per cent.)		Excess success in open treatment (Per cent.)
	Open	Closed	
AB . . .	78	67	11
X . . .	61	56	5
CD . . .	38	28	10

NOTE: Although risk classes vary, each class shows higher success rate for open treatment—even the worst risk class emerges from open treatment with enhanced success experience.

Validation. Results of applying Equations to Different Sample going into Borstal Eighteen Months later than that upon which Experience Tables were Based

Prediction of risk	Number observed		Actual rates
	Success	Failure	
AB (75%) . .	90	30	75%
CD (30%) . .	27	65	29%

NOTE: It seems that results are fairly stable over short periods of time.

Items which 'Came out' of the Analysis with Significant Weightings—i.e. the results of solving equations, not of judgment in any form

Previous convictions (weighted by type)	Max. weight	45.0
Longest period in any one job	Max. weight	11.7
Living in industrial area (objective index)	Weight 0 or	8.0
Living with parents or not	„ „ „	7.5

NOTE: These four items contain as much predictive power as the sixty or so items extracted from the files. Other items might 'predict' but were not as efficient as those quoted above with the weightings given.

First Results
(Applying weightings above)

<i>Risk class</i>	<i>Raw Score range</i>	<i>Results of Training</i>		<i>Rate per cent. Success</i>
		<i>Success</i>	<i>Failure</i>	
A	0-9.9	47	7	87% } 75%
B	10-14.9	48	24	67% }
X	15-23.9	96	65	59%
C	24-39.9	28	54	34% } 30%
D	40 or over	2	14	13% }

NOTE: Centre group (X) was not regarded as 'predicted' by this equation, and a second equation system was set up to discriminate failures in this group.

II. C. P. HILL (LONDON)

I must follow Mr. Wilkins in prefacing my remarks with two disclaimers, first that though I am described in the agenda as a member of the Home Office, I am about to express my own private opinions and any resemblance they may bear to the official views of the Department is purely coincidental; and second, that since the object of a symposium is to promote dissension and provoke discussion, my attempted demolition of Mr. Wilkins' arguments does not imply any lack of appreciation of the value of the work we have been engaged on together.

The prediction techniques which Mr. Wilkins advocates are based on the proposition that if certain indicator factors are demonstrated to be reliable predictors of re-conviction, it does not matter for practical purposes whether they are causally connected or not. The Ohlin technique of prediction for

parole, for instance, is claimed to have an extremely high degree of validity in Illinois, but not to demonstrate a causal relationship between the indicator factors and the behaviour of the persons paroled, or to be valid elsewhere.

The ultimate utility of such technical apparatus is confined to the making of decisions in particular circumstances. 'Prediction' answers the question, *e.g.*, whether the probability of re-conviction is less if the individual before the court is sent to Borstal or to prison, by statistical reference to the observed behaviour of groups of people, in one of which the individual is presumed to be included. This knowledge, if it is used in the right way, can certainly be of assistance to the court in the direction of finding the best method of treatment, or to others concerned with such a choice within the penal system.

But re-conviction statistics alone are no more than one objective criterion of post-treatment behaviour. They do not provide a scientifically accurate answer to the question whether treatment has prevented further crime. There may be—we have no means of telling—a significant number of offenders whose experiences in the penal system have led merely to a successful determination to take sufficient precautions to avoid detection next time; for some years the number of indictable offences cleared up has been about 47 per cent. of those known to the police.

There is the further consideration that a bad previous record tends to emerge as one of the most reliable predictors of failure of any method of treatment as measured by re-conviction and the result in practice may be that the adjudicator is faced with a set of predictions in which all possible courses are unfavourable. This may be correct, but is unhelpful.

Statistics (which are abstractions) can never do more than illustrate relationships; they do not demonstrate causal connections but can suggest where they should be looked for. Nothing can be got out of statistics that has not been put into them: their purpose is to arrange facts in ways that can be reasoned about. They have tended to dominate our outlook because they are the necessary instruments of the fundamental researches which are required to ascertain and select the facts; and in the field of delinquency all too little in this way has yet been done. But they are not substitutes for the demonstration of causal relationships which constitutes scientific knowledge; and other methods of research must be employed for this purpose.

To discard the concept of causation is neither necessary nor helpful: and it is equally mistaken to claim that to predict is the same as to control. For example, the position of the hands of a watch is normally a pretty reliable predictor of the sun's zenith under certain fixed conditions. But if you forget to wind the watch, or drop it in the bath, or travel a few hundred miles east or west, the relationship between the position of the hands and of the

sun is quickly lost; and no amount of twiddling the hands will control the altitude of the solar disc.

The defect of these techniques is that they disclose no reason why offenders behave as they do, and afford no assistance in discovering how to improve the treatment of individuals. If, moreover, the success rate falls below what can be regarded as satisfactory, the indicators, having no demonstrable causal relationship with what they indicate, simply cease to be valid and give no help in remedying the situation. To give a simple illustration, a green light at the traffic signals is an indicator of high statistical reliability that it is safe to drive across another road. The causal connection is a quite complicated affair involving the correct working of much electrical and other machinery and the social acceptance of a number of rules of conduct only one of which is obedience to the signal issued by the machinery. If any part of the chain of causation breaks—if, for instance, the signal mechanism goes wrong and all the lights show green, or a drunken driver disregards the red light, or a car's brakes suddenly fail, or vehicles trying to turn right block the crossing—the green light ceases to have any validity as regards the chances of driving safely over the crossing and offers no clue to remedy the situation. So, too, if methods of treatment or social conditions change, the whole prediction system may need re-working, and a completely new set of indicators may emerge. Prediction tables are pragmatic, a short cut; their danger is that they are empirical rather than scientific. We do not know why they work; and therefore cannot correct them when they go wrong except by a complete re-observation of the group phenomena we are dealing with. Research which aims at establishing scientific knowledge cannot stop short at a mere observation and arrangement of phenomena.

Whatever contribution statistical prediction techniques may make towards securing that offenders are subjected to the form of treatment most likely to benefit them, they will not help us to improve the treatment itself. One main problem in delinquency research, I have always felt, is how to reform the hard core of delinquents—the 20 per cent. or so who at present appear to pass through all forms of treatment untouched. We need to know not only that they fail, but why they fail—not only that they were subjected to a certain form of training, but how bad they were when they started and how much better it has made them. These are qualitative questions which cannot be answered by re-conviction statistics alone, and I should have thought it was important to know the answers for all forms of treatment intended to be reformatory, whether for juveniles or adults. It would therefore seem that some form of psychometric analysis should be a principal object of research. This implies the other main approach to research, by intensive studies of case histories, to elucidate the causes which lead human

beings to behave as they do. I do not wish to embark on the philosophy of causation, but I mean by causes, in this connection, the states or events that uniquely precede and are invariably followed by other states or events, not 30 per cent. or 70 per cent. of the time but all the time; and by reasoning about them I mean the process of reconciling our knowledge with all the rest of our knowledge about the universe and its contents. To discard the concept of causation is to discard the rationality of the universe, and to do this enhances neither the claims of prediction techniques nor the possibility of applying them rationally.

III. T. C. N. GIBBENS (LONDON)

By way of introduction, I should like to say that I think Mr. Wilkins over-stresses his point about causality. We can surely accept that one does not need to assume a causal connection between a good predictor and the outcome. Yet, in practice, the choice of possible predictors is always influenced by under-current ideas about causality. He does not say whether he investigated the colour of the offender's eyes in relation to outcome. If he did not do so then, presumably, it was because ideas of causality led him to think the question was likely to be irrelevant.

The psychiatrist who works with delinquents is always more deeply concerned with prognosis than other psychiatrists because he is required to make recommendations to courts which are based on prognosis, whether or not he recommends any treatment. Yet how rarely does he check his opinions by any thorough investigation of the outcome of all his cases. It is very gratifying to find proper studies of after conduct being carried out, quite apart from any special theories underlying them. In medicine, of course, studies of prognosis have proved a royal road to much of the understanding that we have, whether or not the causes are understood. In the end studies of prognosis have usually led to greater knowledge of causes.

From the psychiatric and psychological standpoint, there are two relevant aspects: What help can the psychiatrist obtain from prediction studies? And, secondly, what can he contribute to them?

On the first issue, prediction studies may help us to a limited extent to investigate causes. We have been told quite rightly that predictors are not causes. And indeed a little reflection will suggest that they are very unlikely to be causes. To be effective they must predict for a wide range of conditions: hence they are likely to be aggregates of separate causes or to represent factors which are common to a number of causes. Such predictors as have been found useful—such as previous criminal experience and poor work record—have usually seemed to be large aggregates of this kind.

One might take an example from life insurance practice, since a good deal more is known about the cause of death than about the cause of recidivism. The predicting factors considered in life insurance work are age—rather obviously—and, more recently, excessive weight. Now excessive weight is not a cause of death, but it probably is a common factor in a number of very different pathological conditions which all influence weight in the same direction, and it is presumably a simple way of taking them all into consideration. Nevertheless, if a predicting factor is proved to be continuously efficient, then it is reasonable to use it as the starting point of research into causes.

The main value of prediction to the psychiatrist, however, will probably lie in another direction. If we know the prognosis of a group or category of offenders, then we are in a better position to study the possible reason for deviations from this prognosis. For example, the neurotic offender should be liable, from the psychopathological standpoint, to continue to commit offences since his motivation is irrational and experiences may be stressful for him although quite neutral for the ordinary individual. Yet, personally, I find this a difficult question to be sure about because neurotic offenders commonly come from the 'best risk' category, *i.e.*, they are often first offenders, with good work records, and from homes which have the superficial appearance of being quite good. If we know that only one in ten of such offenders goes on to commit further crime, we have a base line against which to check whether it is the neurotic offenders in this particular category who are mainly responsible for the reconviction rate, even though the rate is better than for that of offenders in general.

The second question is: How can the psychiatrist help prediction studies? The predictionist, if that is the word, is concerned with behaviour, especially of the sort which is easily categorized. But the psychiatrist is wedded to a model of the basis of behaviour which supposes that behaviour is based on instincts and drives, and that these are expressed through a personality which is at least in some respects fairly constant. He is bound to feel that the assessment of drives and of personality will produce the most reliable guide to behaviour, though not, of course, a complete guide.

The predictionist presumably has no objection to this aim. He will reply that, if tests of intelligence, personality and drive can be shown to be reliable measures, he will gladly use them. And we have to admit that so far little has been achieved in constructing such measures. But it seems certain that they will be used in the future to an increasing extent. It must not be supposed that they must be tests with machines or on paper. The interview can quite well be used provided it can be shown that different interviewers can reach the same assessment reliably

The first attempt at the use of personality assessment has been made by Dr. Ogden and his colleagues in the Camp Hill Survey. You will remember that he showed that personality types of delinquents were related to recon-victions. It was interesting that the best prognosis was found in the largest category—those who were in a class which included a third of the subjects. It was as if mediocrity or normality was a good sign and any characteristics which could be categorized in a more detailed way were undesirable in prognosis. It may be that such interview methods should limit themselves to the exclusion of exceptional characteristics.

Once again one is reminded of life insurance practice. Medical examination for life insurance does not assess whether you are fit, or how fit you are, but merely excludes certain categories of major illness. In the same way it may be profitable only to consider certain facts about the personality of delinquents which are fairly obvious but only detectable in a brief interview.

There is another reason why it seems to me that data relating to the examination of the delinquent must come to replace all but the simplest historical data. It is not generally realized by those who are not in daily contact with delinquents that it is impossible to obtain some quite important historical information in some 15–20 per cent. of delinquents. The information is simply not available. Mothers cannot remember which of ten children had the serious head injury. My own experience with the examination of Borstal lads has made this plain. One mother seen recently could not even remember whether her husband separated from her or was killed by a bomb—the information was finally obtained from the lad, who had seen his father in the streets recently. There is, at the same time, never likely to be a time when records will contain the information that is urgently needed and considered relevant in ten years' time.

In conclusion, two further points. First, do we really want to refine methods of prediction? Certainly any refinement must come very slowly. I am reminded of an insurance director who specialized in aircraft insurance. When I commented that presumably he got experts to examine the aircraft for mechanical perfection, he replied that this was just what one did not do. What one wanted to know was the total accident rate and assess the risks on that basis. Mistakes arose principally from trying to particularize and vary risks. In the same way one feels that society's interest is mainly in making sure that in dealing with the delinquent it never loses, and this can only be done by consideration of very broad risks. These risk rates will remain as large umbrellas under which we must go about with our daily detailed work.

Secondly, it follows from my remarks about predictors not being causes that important causes may be concealed in factors which do not in any way

predict. This was brought out by a follow-up of criminal psychopaths which I have recently completed with Dr. Pond and Dr. Stafford Clark.³ The criminal careers of these psychopaths were studied for the five years since they were seen in 1948. It was found that, as Pond found with behaviour disorder children, abnormality of EEG did not predict their subsequent conduct. The reason appeared to be on close examination that, for aggressive psychopaths, abnormality of EEG was a moderately bad sign, but that for inadequate psychopaths it was definitely a good sign. Where all psychopaths were bulked together these opposing tendencies cancelled out and deprived the abnormal record of any prognostic significance.

DISCUSSION

MR. T. S. LODGE: It seems possible that the views of Mr. Hill and Mr. Wilkins (Dr. Gibbens occupying an intermediate position) on causation are not as disparate as might at first appear. Mr. Hill is concerned with finding antecedents which are associated in every case with a particular result—in this context with the failure of treatment—while Mr. Wilkins is content to take the available information and extract those factors which are associated with the result in a more or less high proportion of cases. While, of course, it would be very satisfactory if a causal sequence could be discovered, in a sociological situation it seems unlikely that the existence of such a sequence can be directly proved, so that when it comes to methods the statistical method is on either basis the only one to use, and the direct study of causation useful mainly in suggesting hypotheses for testing.

DR. M. GRÜNHUT: The remarkable advance in crime prediction is only one instance of the influence which modern science with its spectacular achievements has exerted on the study of human behaviour. When modern criminologists aim at a solid empirical basis in their work and test its results by statistical validation, they owe much to the methods which proved so successful in the field of the natural sciences. However, the mere fact that a method is useful in physics does not mean that it is equally suitable for the social sciences. The natural sciences deal with masses of interchangeable units, but the social sciences with groups of individual human beings. Statistical prediction has its place where decisions on groups have to be taken. The Prison Commissioners have to decide whether they should build more open or closed prisons. A country where the probation system is not far advanced wants to know the risks involved in making higher age-groups or certain types of recidivists eligible for probation. In these cases, all that we are interested in is whether the probable number of repeaters would still remain within tolerable limits. But it does not matter in this context who are the ones who succeed and who are the ones who fail in an open or closed prison and on probation or in an institution. If a man dies the day after he has signed a life insurance contract and his widow receives a substantial payment, the insurance company suffers no loss. Insurance companies do not insure single persons but thousands of people, and the amount of the policy is based on an exact statistical calculation of the life expectancies of, say, a thousand men of a certain age with no cancer in their ancestry and so forth. Here, however, the analogy ends. If a probationer relapses into crime the judge's conscience is not calmed by the reflection that probationers of that age-group and similar antecedents have a specific risk of reconvictions. He cannot help asking: *Why* has this man failed? Mr. Wilkins, like Professor and Mrs. Glueck, never pretended to do more with prediction

³ See pages 126–36.

than to provide a solution for a first step in a process of judicial reasoning. Prediction methods determine, in mathematical language, the general risk involved in awarding a form of treatment to an offender who belongs to a specific group. This has the effect that whoever has to take the second step and to give a decision in respect of an individual person 'knows what he is doing'—that is to say, he squarely faces the general risk involved. Unfortunately, however, the way in which prediction is presented often leads to overstatements which put all the emphasis on the first step and ignore the second. In connection with the second step, however, when we are confronted with an individual person there is no room for the thesis that 'the concept of causation is redundant and to be eliminated'. Any form of treatment and even the meting out of the sentence no less than social case-work involve personal responsibility. An element in this personal responsibility towards our erring fellowmen is that we cannot cease questioning: 'Why crime?'

DR. T. GRYGIER: I should like to make a few remarks on the validity of the concept of cause. We are all concerned here with problems of prediction and action; we use research in order to make our predictions more exact and our actions more effective. In this context the concept of cause can be considered as no more than one of the tools. Whether we should accept or reject it will depend on the ultimate saving in labour—a statistical machine is a good tool in this respect—and on the increase in the accuracy of prediction.

No tools are universally valid, *i.e.*, universally useful. They may or may not be useful for some particular purpose in a particular set of circumstances. When Mr. Wilkins rejected the concept of cause he implied its low validity for a particular purpose at a particular stage of procedure. Within his own framework I think he was right.

If we want to determine our action and to predict its outcome *directly*, I agree with Mr. Wilkins that prediction methods based on probabilities are much more reliable than the theory of causality which merely gives certitude to the believer.⁴ If we had one universal cause of crime, the situation would be different. Just as in the physical sciences, we would examine a set of conditions which we could call a cause, and the result would follow with an absolute certainty. In fact we have—or we think we have—a great number of causes of good and bad behaviour. They all 'pull' the outcome in one way or another and we do not know their respective weights. Thus for all practical purposes it seems better to abandon the concept of cause altogether and to deal with factors and probabilities which we can measure. A multi-factor theory of causation defeats its own purpose as a prediction method.

The situation appears to be different when we go a stage back and start experimenting. According to Mr. Wilkins—and again I agree with him—we proceed from hypothesis one to experiment one; as our results never give us a complete answer we then proceed to hypothesis two and experiment two, and so on. We do not have to start at any particular point or even with a hypothesis. We can, for instance, start with an experiment and proceed to a hypothesis. My point is that the concept of cause allows us to make jumps in our procedure, to omit a number of hypotheses and their respective experiments; the laborious process of experimentation and statistical analysis comes only at a much more advanced stage. We must start it at some point, but the concept saves us labour. Even after a number of experiments have been carried out, we can use causation again. We employ reasoning as a substitute for research, and after experiment five, for instance, we jump to hypothesis nine. The concept of cause helps us to do away with *some* experiments—it should never stop us from experimenting altogether.

⁴ I use the word 'certitude' to describe a subjective emotional state of deep conviction which may or may not have a factual basis.

There is also another point at which the concept can be usefully employed. When Mr. Wilkins is starting his prediction research with 'a body of information, a number of possible actions and a clearly defined objective', he is not really interested in *all* the available information and in an unlimited number of possible actions. He selects *relevant* information and actions, and he chooses, consciously or unconsciously, facts which can be considered as causally related to his objective. As Dr. Gibbens said, he would not use the colour of boys' eyes in his matrix of information, because there is no imaginable reason for regarding it as a cause of recidivism, or as being caused by the same factor which brings about recidivism.

To sum up: I regard the concept of cause as a valid research tool in some stages of procedure, in particular in the formation of hypotheses. Once a body of information, a limited number of actions and a clearly defined objective have been selected, the concept loses its usefulness and may even be misleading. Mr. Wilkins is quite right in avoiding it in his own work—which starts exactly at that point.

MR. R. G. ANDRY: Like Mr. Lodge and Dr. Grygier, I feel that my own views on the topic of 'causation' have much in common with some statements which were made by all three members of the symposium. I am wondering to what extent the members and, in particular, Mr. Wilkins agree about the following viewpoint: The word 'cause', I feel, because of its emotive content tends to produce an erroneous notion that we can stop additional research because 'the' cause (or causes) has been found. The concept of cause, as I see it, is only a hypothetical construct (though often a useful and at times necessary one) which allows us to formulate hypotheses and motivates the continuation of research. The word is inclined to be misleading as an explanation or proof. The psychologist Kurt Lewin has incidentally made a similar point when he talked about the relationship of genotype and phenotype. 'What we regard as genotypical at one stage and from one point of view may be phenotypical at some other time in a different context.'

DR. J. O. WISDOM: Despite some of the remarks just made in the discussion, there seems to me to be a real difference of opinion between the opening speakers, in that Mr. Wilkins wishes to deny all utility to the concept of the cause in so radical a way that those who follow ordinary scientific usage could not accept it. That there is non-causal explanation in science is now well-known, and it is extremely important; but this is not the issue. Mr. Wilkins was not trying to justify non-causal explanation; what he was claiming was that what is commonly regarded as causal is not causal at all (what it is does not completely emerge; perhaps it is a statistical shorthand or a prediction summary).

But there is a very clear usage common both to science and to everyday life. If I stumble against this chair and knock it over, my stumbling is a cause of its toppling. Here the antecedent is a cause; but it is so only in relation to a general law. Thus when the summer sun causes the railway track to expand, it can be said to do so only in relation to the general law that iron expands when heated. This usage is convenient and pretty well indispensable. But it does not presuppose any metaphysical notion such as an inner essence of an antecedent which brings about the consequent. It was perhaps against such notions that Mr. Wilkins was tilting. But it would be a pity to throw away the baby with the bathwater; besides it is doubtful if these *bêtes noires* any longer do any damage.

I was glad Mr. Wilkins stressed the importance of the hypothetico-deductive system, though I did not recognize his version of it. He agrees that we start with a hypothesis; but when we reach a point where we get stuck, according to him, we perform an experiment and analyse it, thus obtaining a new hypothesis which facilitates further work. According to the theory of the hypothetico-deductive system, however, the experiment could not be planned or analysed, *or even thought of*, without the guidance of the new hypothesis, hit upon somehow in advance by some stroke of imagination.

MR. L. T. WILKINS (*in reply*): Mr. Hill raised the question of 'successful avoiders'. This is, I think, a more serious consideration when one deals with so-called 'non-delinquent' groups for control purposes. Once a man has a record he is rather easier to catch again than one who has not been caught before, and a lot of crime is for first known offences. There is no doubt that the failures are failures, but the success group is doubtless contaminated by avoiders. From evidence in the full study, it appears that this is not likely to have much effect on the equations, but it might show in the reduction of efficiency. Mr. Hill advocated psychometric methods rather than prediction methods. This seemed to be replacing an efficient tool by one somewhat less efficient. Standardized tests at their best rely on external criteria in exactly the same way as the conventional prediction methods. At their not-so-good levels, they rely upon internal criteria. It is now generally agreed that intelligence tests measure '*what-intelligence-tests-measure*'—that is, there is no external criterion. Personality tests can similarly be built up only by internal relationships, or by reference to external subjective judgments. If we can define what we want to control, it seems better to go straight to that point and to use external criteria, such as 'success' or failure. Control implies prediction, but prediction does not imply control. If we seek general solutions we may be spreading the research effort over too wide an area to obtain any useful results. We may only gain glimpses of the already obvious. We have to concentrate our effort on a small area upon specific points. Dr. Grünhut has said that, although prediction methods are suitable for groups, they tell us nothing about individuals. I have gone into this problem with Dr. Mannheim in the full report. It would require a long statement to demonstrate both how true and yet how false is this point. Quite simply, and inadequately, the position can be summarized by saying that if one works in the delinquency field, one cannot deal with individuals—in the course of one's lifetime one will deal with very large numbers (groups) of individuals. It can be shown that the most efficient way to use the information available is by the prediction method (equation solution), and if anyone gives any particular case in a group a different weighting on the basis of *just this information* (i.e., gives the items different weights from that derived from the equations) he will on average be making a worse decision, and thus over the total number of cases he will deal with in the course of a few years, he will be reducing the number of correct decisions made. But the relationship between 'decision' and individual probability is possible only in terms of defined information. Information not included in the original set of 60 items may have any weight, and the equations do not help us here. The confusion arises to some degree because the equations do not show the items which obtain zero weight, or items which, in the presence of those quoted, can be equated to zero. There may well be other sets of items from the same basic information which will give almost as good a prediction equation, and one might feel tempted to bring these into account in dealing with any one case; but if one weights the four items as shown, and if one predicts success/failure in the terms defined, then the best prediction for any individual will be that relating to the group, or perhaps by a linear interpolation for the individual 'score' against the individual risk. The prediction equation 'ranks' the *individuals* in the sample from the greatest to the least expectation of failure; they are grouped for statistical purposes, i.e., for stability of aggregates.

Dr. Wisdom has, I believe, raised two important points. In the first place he considers my condemnation of the 'causal' concept to be too wide and drastic. I have already agreed, in my reply to Dr. Grygier, to go part of the way with those who want to use the idea of cause. I might go even a bit further on second thoughts and agree that one might, for certain purposes, use the word 'cause' in the layman's sense without getting into trouble. Perhaps it does not matter if people believe in causes. It is only when beliefs reduce the efficiency of methods that I object to the beliefs. Because there is so much confused thought on this matter I would recommend that social scientists ceased

to use the word 'cause' for the next ten years. Such abstention might lead towards an improved methodology.

In the physical sciences, if we are prepared to take the lay meaning of the word 'cause' and to improve it to our purposes by appropriate definitions, we might perhaps reasonably use the word 'cause' scientifically. The process, which I am told is due to Tarski, is as follows. If there exists a 'universal law', and according to this law two events x and y are related to each other such that y is the *logical consequence* of x and this law, then x may be said to be the cause of y .

But the universal laws are summaries of observations and years of patient endeavour. Universal laws are not found until a science has reached a fairly advanced stage of development. Social science has not, so far as I am aware, reached this stage, and I know of no universal laws in sociology; social science should, therefore, not (yet) talk of causes. There is at this stage, too, a more potent reason. In physical experiments x can be separated from x' , x'' , that is, the input information can be uncorrelated. Experimental methods in the physical sciences can therefore isolate the variance due to x , x' , x'' in y . In the social sciences it is usual for x , x' , x'' to be correlated and we can talk only of 'joint effects'. Thus we may obtain *efficient* solutions, but not *unique* solutions. This leads to my need to invoke 'decision' rather than 'explanation'. If explanation is the only end of science then my methods are not scientific. I would accept that, and go on to accept that social science is not a science. These prediction methods are a form of 'linear programming'—of linear guides to decisions—a simplification, not an explanation; they get rid of *inefficient* information but not of 'meaningless' information.

If I knew what my critics meant by 'explanation' and 'meaning', I could be more precise; but I have, I think, used these words in terms of the general meaning likely to be attached to them and not in any special way. My critics have accused me of trying to import the methods of the physical sciences into criminology. On the contrary, I wish it were possible, but I insist that at the present level of our knowledge there are *few* areas in social science where the design of experiments appropriate for and powerful in the physical sciences can be employed. Where they can be, they should be, and elsewhere we should not be content to do nothing, or guess, but obtain what guidance we can from such methods as can be used.

Dr. Wisdom's second point worries me more. He seems, in his last paragraph, to elevate the hypothesis to a central rôle. I thought that one of the features which decision methods had in common with the hypothetico-deductive system was that both regarded hypotheses as cheap and plentiful? The best evidence which I have been able to find does not prove direct but is a very interesting statement by Popper (*The Open Society* P 210) and, because this says some things I would say much better than I could say them, I quote: 'The only course open to the social sciences is to forget all about the verbal fireworks and to tackle the practical problems of our time with the help of the theoretical methods which are fundamentally the same in *all* sciences. I mean the methods of trial and error, of inventing hypotheses which can be practically tested, and of submitting them to practical tests. In other words, a *social technology is needed whose results can be tested by social engineering.*' (Popper's italics.) My impression from this quotation is that I am not alone in regarding hypotheses as cheap, and that it is better that one should not spend much time in polishing hypotheses and perfecting theories, but should seek to test them at the earliest moment—that, in fact, without experimental evidence, any theory and any hypothesis is as good as any other. Decision theory, I believe like the hypothetico-deductive method, enables the rapid clearance of deadwood and the modification of hypotheses ready for the next step, *and step by step*. Of course the approach in the developed physical sciences will be different from that in the social sciences. In the physical sciences there exists already such a body of knowledge that a problem may be simplified or—more correctly—limited, and because of this limitation (resulting from knowledge based on earlier repeatable experiments) more powerful

models may be employed. In the laboratory a model which gave a 'decision' answer without an 'explanation' would be unnecessary because in most cases we could have both if we used an efficient design. I am not saying that the 'decision' approach is the only one, but it is one approach, and one which can be used more widely than some others.

Dr. Grygier says that I would not consider using information regarding the colour of a boy's eyes. I might not do so, but in principle I see no reason against examining this information provided the cost of doing so is as small as, or smaller than, the likelihood of any useful result—in this case both might be small! In fact I did notice in dealing with Approved School boys that one psychiatrist had commented on the characteristic 'big ears' which seemed to be present in a fair number of cases. This information was, therefore, examined in a pilot study, but, as may be expected, the results were not striking enough to justify entry into the full analysis. I think one might be able to jump steps in experiment by the process of reasoning, at some stages, but unless these missed steps are included in some way in later experimental work, there must always be some doubt. Quite often later steps may be taken to have filled in gaps, and in these cases I would eventually be satisfied!

I agree with Mr. Lodge that possibly my views are not really very different from Mr. Hill's. Mr. Hill is considering the problem from the point of view of the administrator. His use of the word 'cause' is not the scientific use of that concept. Outside scientific work I would raise no objection to the use of the word in its colloquial connotation, but, although I believe that the future of criminological research will depend very much on the co-operation of scientists and administrators, we must be very careful to keep the meanings attaching to the same words quite clearly separated when we are using them in the two different settings. When a psychologist uses the word 'intelligence' he does not mean the same as when the layman uses the same word. It is when the scientist uses words like 'intelligence', 'cause' and many others that we look to interpret them in a different light. In this connection my viewpoint is best expressed in the footnote by Dr. Grygier in relation to the causal concept, '... gives rise to a subjective emotional state of deep conviction in the believer. . . .'. Such emotional states may *motivate* the scientist, but cannot be the grounds for his acceptance of one 'explanation' rather than another. In conclusion, and mainly in answer to Dr. Gibbens' major argument, I would not claim that the prediction tables so far produced are as good as they could be, given good data and, perhaps, better theory. I am defending the method, not the particular results we derived. The weightings which we found are not the end—they may serve an interim purpose related to decision making, but they may only be the beginning. Instead of questioning the method or the weights, a more profitable line for theorists might be found in taking this first simple solution and suggesting expansions of the equations.

PREDICTIVE RESEARCH. A CRITICAL ASSESSMENT OF ITS PRACTICAL APPLICATIONS¹

By R. L. MORRISON (LONDON)

INTRODUCTION

Varied opinions have been expressed on how far current predictive methods can be usefully applied by field workers responsible for the diagnosis, treatment or training of delinquents. The following evaluation ² is essentially a personal one and is made from the point of view of a clinical psychologist working in the field of Borstal allocation.

My comments are in the first instance negative. None the less they are motivated by a belief in the ultimate usefulness of the predictive method for the field worker, a need for advice on how present results can best be put to practical use and a desire for a better, or at least a different, predictive approach in future. This leads finally to an attempt to find a more positive perspective for further predictive research.

UNDERLYING ASSUMPTIONS

In order to examine two examples of the 'Uses of Prediction' given by Mr. Wilkins ³ (13) let us accept meanwhile certain features of his theoretical position. He bases this on 'decision theory' and quotes (14) Lindley (5) as follows: 'Any experimental result, even in the purest branch of science, is a guide to future action and the function of statistics is to assist in deciding what the future action shall be.' He goes on: 'Causes are not considered and cannot be considered. We have a configuration of information, a configuration of decisions which might be made and we have an idea of the good we wish to maximize. The problems are solved within this framework.'

¹ I am indebted to the Prison Commissioners for permission to publish this article though this does not imply that it necessarily represents official views.

² Based on a Paper intended to form the basis for a discussion between the author and Mr. L. T. Wilkins at the Study Group of the Royal Statistical Society, in March, 1955.

³ I am indebted to Mr. Wilkins for his generous permission and encouragement to criticize freely material from two of his recent papers (13, 14). These are based on his work with Dr. H. Mannheim on Prediction Methods in relation to Borstal Training (6).

THE USES OF PREDICTION

Within the above frame of reference Wilkins continues: 'Although prediction tables say nothing about "causes" of crime, they enable decisions to be taken on a rational basis.' He then gives two examples which can be regarded as typical of current predictive conclusions and their presentation and makes typical claims for their usefulness as an aid in making decisions.

(1) *Comparative Success Rates in Open and Closed Borstals.* After describing how, on the basis of four items, Borstal trainees can be classified into 'risk categories' with corresponding predicted success rates (*e.g.*, an AB group with a 75 per cent. expected success rate, a middle group (X) and a CD group with a 30 per cent. success rate), Mr. Wilkins continues: 'Since our 1946-7 sample covered both types of Borstals we were able to consider the difficult question of whether open Borstals were more effective than the closed. When we examined the success rates for the two types of training separately for our 1946-7 sample we found a success rate of 58 per cent. for the open and 36 per cent. for the closed institutions. We also found, however, that 74 per cent. of the (AB) group and only 39 per cent. of the (CD) group went to open Borstals. We should expect, therefore, a better success rate for the open Borstals—they had better material to work with. But would the difference of 22 per cent. more successes from open Borstals be explained by this difference in the class of lad received? We may examine this by calculating within each risk group the success rates actually achieved. The results of this analysis are shown in Table III.

TABLE III

Comparison of Success Rate for Open and Closed Borstals Using the Prediction Tables ⁴

Risk Category	Success Rate		Excess successes in open Borstals
	Open Borstals	Closed Borstals	
AB . . .	78%	67%	11%
X . . .	61%	56%	5%
CD . . .	38%	28%	10%
All cases . .	58%	36%	22%

⁴ Such a comparison by classes of risk allows for the fact that a higher proportion of the good risks are sent to open Borstals.

Thus we see that the best group (AB) in open Borstals succeeded 11 per cent. more often than lads from the same risk group who went to closed Borstals; similarly the worst group (CD) who went to open Borstals succeeded 10 per cent. more often than lads from the same group who went to closed Borstals. The difference between the open and closed system of training is, after allowing for the type of lad, reduced from an over-all difference of 22 per cent. to about 8 per cent. It seems, then, that open treatment was better than closed, but there may be other features than the type of treatment which gave rise to this improvement. Perhaps the open Borstals had better premises or better staffs, or perhaps some other factors accounted for the difference.'

A number of points, some relatively minor, immediately arise, for anyone directly involved in this field situation:

- (a) Causal implications creep into the presentation of the findings and the need for qualification of these is immediately perceived. To recognize the possible relevance of other factors such as premises or staff takes one little further since no realistic or practical means are available for testing such speculations.
- (b) Once one admits the possibility of 'other factors' which might explain the obtained differences in success rates, the field worker is tempted to resort to a belief in 'intangible' risk factors operating in addition to those included in Wilkins' calculation of risk.
- (c) Any meaningful explanation of the findings is then regarded as requiring recognition of the existence of an allocation process since those allocating would consider that they take account in a total, intuitive way in each individual case of such 'additional' risk factors and that if these could be isolated, the differences in success rates might vanish. The suggestion here is that if total risk and not simply risk as operationally defined were to be taken into account, no significant differences might then emerge in favour of open treatment.
- (d) The general argument here would appear to be that, if within any risk group a relationship is claimed between any factor (such as type of training) and success, we cannot accept this at its face value if there is reason to suspect the operation to an unknown extent of other determinants also associated with both type of training and success (*i.e.*, other relevant but 'intangible' risk factors). It would appear that any obtained relationship between the factor under consideration and success might be either an inflated or a blunted one, this depending on how the various risk factors were associated between themselves and with success.

- (e) Some of the above points might be regarded as invalid or irrelevant from the strict 'decision theory' position; others might be regarded as not demonstrable either way. Assuming then that the 8 per cent. greater success rate in favour of open Borstals remains, what is the exact value of such a conclusion either to the makers of policy or to the Allocation Boards who have to take decisions in individual cases?

In many instances decisions on open or closed training have of necessity to be based on practical considerations which make reference to risk categories unnecessary. Where this is not so and the decision remains in the balance it is of limited value to know, for example, that a lad categorized as an AB risk will have a 78 per cent. chance of success if he is sent to an open Borstal, and a 67 per cent. chance if sent to a closed. Such differences are not atypical of predictive findings.

Even if the operational problem could be expressed in the relatively simple way we have just done, there are real practical difficulties in deciding how great such obtained differences must be before they are entitled to weight (and how much weight relative to other considerations) in contributing to its solution.

The field situation is, however, even more complicated since it involves the allocation of an individual to a particular training institution and this requires a balancing of many factors of which the open/closed question is only one. In most cases it is not possible without being artificial to consider this question in isolation at all.

If prediction methods are to meet the reality needs of such decision situations they will have somehow or other to avoid the use of over-simplified abstractions from complex decision processes without landing at the opposite extremes of producing either a mass of simple prediction tables (with consequent problems of relative weighting) or multi-variable tables too complicated for practical use. Either of these would arouse considerable field resistance.

- (f) Again, the way in which the predictive information has been presented in the above example probably begs certain important questions. Many people would argue that a statement in terms of individual chances is not strictly the same as one in terms of probable group outcome. Grünhut (3) sums up this problem of how far group statistics can legitimately be used in individual cases. 'It is still an open question whether we are justified to decide on a man's destiny by assuming that because he exhibits symptoms similar to those of a group out of which 80 per cent. are persisting in crime, he himself

has a chance of 4 to 5 to belong to the 80 per cent. persistent criminals and not to the 20 per cent. who become straight.'

- (g) One might attempt to sidestep these problems and present the predictive conclusions in general form. Allocation Boards, however, would not be greatly impressed by advice that they should allocate lads wherever possible to open Borstals as they would regard that as their policy in any case. This is no doubt beside the point but the field worker is not alone in being easily tempted into accusing the statistician of proving the obvious.

(2) *The Effects of Different Periods of Detention.* On this question Wilkins writes: 'Another example in which use was made of the prediction tables was in considering the effect of different periods of detention. Borstal detention is not for a fixed period, but may vary between 9 months and 3 years. On our first examination of the effect of the period of detention we found, not unexpectedly, that the longer a lad was detained in training the less likely he was to succeed. This result was, of course, due to the worse risks being given longer periods in training. When we examined these results within risk classes (in the same way as above for open and closed Borstals) we found that periods of detention had no effect upon the success rate for the same risk group.'

This sort of statement again appears unsatisfactory from the field point of view and even from Wilkins' own starting point:

- (a) Despite his insistence on 'decision theory', the phrase 'had no effect upon' suggests causal relationships whether these are intended or not.
- (b) One could hardly blame a Borstal Housemaster, for example, if he were to take these findings to mean that for a particular risk group it does not matter how long a lad is kept in training (or how long he is kept beyond some optimum period). He would dismiss such a conclusion as contradicting both his experience and commonsense.
- (c) This merely emphasizes the importance of the verbal form in which statistical findings are expressed, *i.e.*, the way in which they are 'interpreted'. This becomes clear from an examination of the detailed figures of success rates for different risk groups serving different periods of training at open and closed Borstals considered separately.

From these figures no general trend emerges, a situation very different from the previous example when a consistent pattern of differences was found operating over all risk groups. This makes it extremely difficult to provide any simple generalization which covers all the observed phenomena. Perhaps the only useful thing is for the statistician to recognize that his findings are too complicated

to feed back into the field in simple, unqualified fashion and to say that 'no relationship has been established' between length of training and success or that he has no pattern of results on which he can base general advice.⁵

- (d) It is even more difficult in such circumstances to erect general hypotheses which are psychologically meaningful. It also follows that any hypotheses which one erects to cover parts of the data will be extremely tentative ones if these can only be reconciled in highly involved fashion with other partial hypotheses. This would seem to be the case here.
- (e) Even without the above objections the field reaction to such statistical findings would still be to regard them as true only in the limited sense of the experimental definition of 'risk'. The training staff would again insist that there are other practical as well as intangible risk factors perceived in the course of training and that these are taken into account in the decision on when to release. They would argue with conviction on the probable effects of this on the success rate whether taken overall or as analysed out by the statistician for his various risk groups.
- (f) The general argument is again that the obtained results are a product of the inter-relationships existing between the defined and undefined risk factors, length of training and success and that a fully meaningful interpretation of such results must take account of these, with special reference here to the constancy of such relationships over the various risk groups.
- (g) In its most practical form, however, the field problem remains unchanged. If we fall back on the differential success rates (or absence of significant differences) which occur within the smallest units of

⁵ I am greatly indebted to Mr. Wilkins for providing the necessary detailed information which I have taken the liberty of arranging as follows:—

Length of Borstal Training and 'Success'.

<i>Length of Training</i>	<i>Open</i>			<i>Closed</i>		
	<i>AB</i>	<i>X</i>	<i>CD</i>	<i>AB</i>	<i>X</i>	<i>CD</i>
Above aver.	60	55	40	67	58	27
Below aver.	86	67	37	67	54	30

Entering this table at the top left-hand cell we read that 60 per cent. of AB risks doing above average periods at open Borstals and 86 per cent. of those doing below average periods are successful on discharge: and so on.

analysis (even on the most striking example, the 26 per cent. greater success for AB risks doing a 'below average' period at open Borstals) as far as the operational usefulness of such a conclusion is concerned we are back where we left off in discussing the previous example, with the additional complication of difficulties in reconciling the various risk group findings. The implication here would appear to be that AB risks could be released earlier from open Borstals. Even if one accepts this as a general statement the fact remains that it is not an easy matter to assess the exact relevance of such a finding for deciding on when to release an AB Borstal lad. The information provided is neither so simple nor so 'practical' as might at first sight appear.

(3) *The General Claims Made for Predictive Methods.* The traditional claim made for predictive findings is that they will help the Courts by enabling them to base sentences (of Probation, for example, as against Approved School or Borstal) on a more rational and scientific basis. This statement loses most of its force if it is watered down to refer to 'sentencing policy generally' and it appears, though this is not always clearly brought out, to refer to the sentencing of individual cases.

Much of the argument developed over the two examples from the Borstal field applies equally to the Court situation where the decisions to be taken are even more complex than those just discussed. The current tendency is to over-sell predictive findings without really demonstrating their exact practical applications, and rigorous criticism, however destructive it might appear to be, seems necessary as a corrective.

It seems worth emphasizing that, even with the most favourable bias towards the predictive approach, most field workers find genuine difficulty in making effective use of the kind of findings now being made available to them. Even if one suspects that such field protests are to a large extent emotionally based, rationalizations of a reluctance to change or admit the inefficiency of subjective judgments, such resistances will not be lessened by the attitude, expressed or implied, that predictive findings ought to be found more useful or the suggestion that field workers are not 'trying their hardest'. Without going into reasons, one would suspect that greater resistance will emerge from the Courts than, for example, from the Borstal field. These too will find convenient expression in the complaint that the research findings do not really help in day to day work.

A recent statement by the Gluecks (2) repeats their previous warnings against the mechanical use of prediction tables and points out how they are merely intended as an aid to the field worker, 'to help him see the individual in the perspective of organized experience'. The modesty of this aim is often

lost sight of and a greater insistence on it would greatly reassure field workers. Both in the Courts and the Borstal field, predictive work can assist to some extent in the taking of decisions but more than that cannot be said. Rose (11) sums the position up in these terms: 'Prediction tables may, in the future, aid in allocating offenders, in a broad way, to different types of treatment, but this is as yet far ahead.'

Meanwhile predictive workers and their potential 'users' are not yet sufficiently 'in touch'. Perhaps negative criticism of the kind expressed here will do little to close the gap but it should certainly help to assess its extent. (Some more positive suggestions are made below for dealing with such problems.)

BASIC REQUIREMENTS OF PREDICTION TABLES

As a result of his own experience, Wilkins (13) has laid down certain general principles for the construction of workable prediction tables. Even where one agrees with his main conclusions, a number of points seem worth debating:

(1) Admitting the need for concise, simple mathematical tables, and a minimum of these for the busy 'user', is it really possible to express adequately in this way the kind of predictive information which could be given a heavy weight in field decisions? There would appear to be a real dilemma here. The more one presents results in condensed fashion in order to make them manageable for the field worker, the less these may appear to be of real value to him in taking complex decisions in individual cases. This problem is often obscured by a failure to recognize that while a decision may appear simple in terms of the available alternatives the process of reaching a choice may be a very complicated one. Enough has already been said above on such difficulties.

(2) Admitting the need for keeping tables up to date by periodic or continuous revision, can this be done practically and economically when increasingly effective feed-back of results into the training field accelerates treatment change?

Wilkins (14) has produced a stimulating and optimistic account of how the statistician has already begun to tackle such problems using dynamic models which give rise to such techniques as sequential sampling and multiple regression and has suggested how even more promising techniques may emerge in the future. So far, however, such technical advances would appear to have been confined to situations where field changes have proceeded in relative independence of predictive work. So far there has been little real penetration of predictive findings into the field situation and a whole area of discussion remains on what would happen if communications were

improved and there developed increasingly rapid and effective feed-back of the kind of information which made possible treatment change and accelerated this process. Whether such new 'time lag' difficulties can also be coped with by the statistician remains to be seen.

(3) Admitting that tables must be reproducible by others, can the resulting emphasis on the need to use factual data rather than subjective judgments really be left at that? A psychological bias to one's approach leads one to suggest that the reverse will have to be tried to provide a basis for prediction which will do justice to the complexities of criminal behaviour. This, however, while perhaps leading to more sensitive predictive techniques eventually, might, at least in the first instance, lessen their direct practical applicability. Both of these points are discussed below. One basic confusion, however, must be removed. Wilkins (15) regards subjective judgments as 'essentially unrepeatable'. This is not strictly true from the operational point of view. What is necessary is that they should be of sufficient proved reliability and validity for their purpose.

SUBJECTIVE APPROACHES AND THE CONDITIONS FOR THEIR INCLUSION

Wilkins (13) again provides the starting point. Of his Borstal work he writes: 'No subjective judgments proved of value in the predictive equation although a number of these were examined', and again: 'No subjectively based information was found which gave information *additional to* that obtained by the efficient statistical treatment of factual data'.

A number of other British workers (Rose (11), Spencer (12), Norval Morris (8)) have also attempted to make use of this kind of information in their researches without finding it to be very effective for their purpose. This is not really surprising since it is largely the result of using for research purposes subjective material which is adequate to some extent for certain operational uses but completely inappropriate for a kind of analysis for which it was never intended in the first place. Certain qualifications are in order here. In many instances the data which is available in the official documents used in such researches is too unreliable for efficient operational use either, but this is no consolation to the research investigator. So-called 'factual' data are often subject to various sources of contamination and are also, if to a lesser extent, unreliable.

Such a situation is clearly relevant when considering the limited conclusions which have so far emerged from the kind of objective data so far used by such investigators (or even by the Gluecks). The difficulties, more accurately, centre round the kind of information so far found usable by research workers, especially in view of the traditional statistical techniques available for its treatment.

By 'limited conclusions' one refers essentially to the kind of statistical findings (co-efficients of contingency of around .4 (1) and multiple correlations of the order of .5 (6)) based essentially on such factors as work record, number of previous convictions and certain limited aspects of family life. Findings of this order leave so much of the variance unexplained that they are of limited value for individual prediction. It is no wonder that, in these circumstances, field workers resort to arguments in terms of 'intangibles' that have been left out. The remedy no doubt lies partly in the refinement of 'factual' data and the improvement of statistical methods, but one would suggest that if further predictive accuracy is to be obtained it will be necessary to come to terms also with the problems of expressing subjective judgments, not only on such usual areas as work and crime but in the field of personality assessment as well. It must be admitted that the recent research shows agreement about the former measures. Both Rose and Spencer, as a result of what one would imagine to have been 'bitter experience', are under no illusions about the incompleteness and crudeness for research purposes of even the factual data available in records designed largely for administrative use. Considerable confusion, however, exists over the possibilities of including the 'intangibles', the latter term being taken to mean no more than 'factors of behaviour and personality which have so far not yet proved capable of expression in appropriate "measurement" form.'

It is unfortunate that up till now, in this country, the pattern of work has so often been that of the individual investigator 'doing research' on whatever material is available and, in the process, being driven into make-shift techniques. Often such workers, as the result of limited research resources, have been forced into drawing their material on behaviour and personality at second hand from paper instead of at first hand from people. Where the latter has been attempted the investigator has often not been psychologically equipped for making his own assessments in these areas.

At the risk of dogmatism one would lay down the following criteria for the effective development of the subjective approach in research: (1) 'Psychological' data on the areas of behaviour and personality to be provided by suitably qualified specialists and field workers who have been (2) specifically trained and briefed for the particular research project; (3) expressing their findings in a form suitable for subsequent analysis and statistical manipulation, *i.e.* (i) using dynamic concepts of personality, etc., on which to base (ii) rating scales and other devices (including, possibly, tests), (iii) whose reliability and validity have been previously checked, (iv) and/or are capable of being (further) examined within the limits of the research investigation itself.

It must now be admitted that, if these criteria are to be applied rigorously, most of the necessary tools and techniques are 'still to seek'. This may appear unnecessarily pessimistic in the light of the increasing development of psychological tests, questionnaires, personality inventories and so on. Such methods, however, are not likely to provide simple solutions to the problems under consideration.

Intelligence and such special abilities as are measurable with reasonable accuracy are, it would seem, unlikely to prove of great value as predictive indicators, such evidence as has emerged suggesting that these factors are of little general significance in precipitating or predisposing to delinquency or in determining recidivism. In the field of personality assessment (on the basis of attitudes, needs, drives or other dynamic concepts) the available instruments (questionnaires, projectives, etc.) are not yet sufficiently developed to be able to produce precise, reliable and valid measurements for use in the kind of predictive work under discussion, especially within delinquent populations. The latest Glueck study (2) appears to support this view, even when the aim was the broader one of screening out potential delinquents, and great emphasis is laid on the time and specialized skill needed for the use of techniques such as Rorschach.

Perhaps, and these are really personal preferences, the most immediately fruitful approach would be one based on rating and scaling devices or the use of dichotomous classifications. Wilkins himself appears attracted to such possibilities from the point of view of statistical techniques which are emerging for handling attributes.

The crucial difficulty in personality research which lies behind the problems discussed has been recently referred to in such terms as 'plague' and 'dilemma' (4). 'Many concepts which seem useful on a purely understanding or intuitive level are extremely resistant to scientific operational test. On the other hand these concepts which seem to be most immediately amenable to operational formulation and quantitative expression seem to have less "explanatory" power for the clinician.'

Marcus (7) has recently expressed a similar point of view on the necessity of dynamic concepts of personality for the investigation of criminal susceptibility. He describes this latter problem as one of 'isolating the dimension(s) along which statistically significant differences can be found'. His references to psychologists 'making good guesses' at these dimensions or continua and 'using their tests or devising fresh tests, to make the appropriate measurements' implies that such basic work has yet to be done.

Recent research workers have also emphasized the need for getting beyond the use of 'factual' data. Rose (11), who set out on an investigation largely modelled on an earlier Glueck study, reveals in his conclusions an

obvious searching towards an approach more concerned with personality dynamics. He finally comes to regard data which merely record external events or individual actions as of limited value and even misleading when compared with the attitudes and emotions operating underneath. 'One doesn't want to categorize cases in terms of events or actions but in terms of reactions to events and causes of actions and every classification of events and actions merely conceals reactions and causes which are certainly diverse and may well be conflicting.'

The current tendency in delinquency work in this country has been to approach such problems through various kinds of personality typing. This has been partly a matter of choice but has also been to some extent the result of attempting to make a virtue of necessity, an argument which is limited in its application and tends in the extreme to lead to an avoidance of the underlying difficulties of working out suitable operational concepts for the expression of processes operating with the personality.

Even where the starting point is the idea of 'reaction to' stimuli such as events or people, the types or categories which emerge (Rose (11), Ogden (10)) tend, on the whole, to be descriptive in their emphasis rather than dynamic. One tends to reach by such methods clusters of common or overlapping traits or attributes, rather than variables the intensity of which can be assessed or theoretical constructs for handling processes for which hypotheses can be erected and tested out by reference to subsequent events. One would suspect that such 'types' especially when used by lay workers would become static, at least in the sense that refinement or development would seem unlikely to emerge from within the category system itself even if it were used in follow-up studies.

Such 'types', however, may prove to be useful first approximations and they are probably of more immediate value to the training field than the classical kind of prediction statistics as an indication of where effort and enquiry should be concentrated. The great advantage of the techniques devised by the Camp Hill Survey team (10) is that, however naive psychologically and statistically its assessments of personality, and however defective these might be in rigorous dynamic conceptualization, they were worked out by group participation methods in which laymen came to terms with such trained specialists as were available within the institution. Such methods will always produce instruments which are more attractive to field workers and consequently more likely to be used. The problem remains of how far one can increase the technical efficiency and sophistication of research within such a framework. Perhaps one cannot have it both ways.

THE PREDICTIVE APPROACH AS A METHODOLOGY

The doubts expressed above on the extent to which current predictive techniques and the form of their findings are of practical field application, and the further difficulties which arise when one considers how far this situation could be improved by the inclusion of a more subjective approach, combine to force a reconsideration of the underlying rationale of the predictive approach. The 'decision theory' position from which we started goes far beyond 'statistics for statistics' sake' but already a number of other assumptions have filtered in with the more dynamic viewpoint expressed above. Most social psychologists would regard the following as a more reasonable statement of their position. 'The aim of social science must be the explanation of complex causal relationships which produce observable end products. Once we have a glimmering of the nature of causation we can look forward with optimism into the era of effective prevention in the criminological field and with more confidence in devising therapeutic techniques.' (9) Perhaps this frame of reference and that of decision theory are irreconcilable. Perhaps one can at least dodge some of these issues meanwhile by regarding the predictive approach (even when its experimental designs are based on decision theory) simply as a methodology (or part of one), a series of statistical techniques for reducing facts to order, for establishing relationships between them, for making possible the erection of hypotheses of a causal kind which would in turn be subjected to verification by other methods (by combined statistical-clinical-operational techniques), *i.e.*, by regarding predictive research as a means of ultimately but not immediately or directly increasing our knowledge of delinquency and how to treat it. To most psychologists the strict decision theory approach appears to rest on a cumbersome trial and error basis and fails to include within itself the means of its own rapid improvement. Some infusion of dynamic hypotheses at some stage would appear from the psychological point of view to be necessary if only in the interests of economy of investigation.

The implication here is that we may have in the first instance to move, or appear to move, away from the position of direct action or operational research into one which appears more artificial and less relevant to the actual conditions of field work. Perhaps the dilemma is more apparent than real, especially if we regard such research as a useful next step if not a pre-requisite to improved operational techniques for routine field use.

THE NECESSITY OF AN INTER-DISCIPLINARY APPROACH

The 'limited conclusions' referred to are no doubt preferable to no conclusions at all. The kind of research so far conducted must also be

accepted as perhaps the only work on such problems which was reasonably possible under the circumstances. Even so, it would appear that the limits of these traditional research methods have been reached. (Recent British research workers would appear to have reached the same conclusions themselves and it is to their great credit that they have already said so in their assessment of their own work. Many of the following points have also been made in one form or another in one or other of these studies.)

The primary need is now for carefully designed experimental investigations by research teams of as broad an inter-disciplinary character as is practical. One would like to see such teams, however small, setting out to collect, for the most part, their own raw material, this and its subsequent statistical treatment being planned from the start to fit each other. At the same time, these would have to be related, as far as possible, to the actual field conditions under which operational decisions are made, the aim being to express results in a form which can be effectively assimilated into the actual process of taking such decisions, and not merely to provide further vague aids to clinical judgment or field experience. Finally, one would hope that a research so conceived would not only put the treatment of delinquents on a more rational basis but would also lead to a further causal understanding of the complex social phenomena involved. (How far some of these needs are irreconcilable can best be decided by making the attempt.)

Such planning can avoid the waste which, as Wilkins (14) points out, occurs when statisticians are regarded as 'salvage operators'. He rightly insists that statisticians must not only help to formulate the right research questions but must also begin to take more responsibility for their findings. It cannot be too strongly emphasized that the link between these two points is the exact design of investigations in terms of actual field (or decision) situations. Rose (11) appears to express a similar idea when he refers to the need to ally constellation of factors occurring in delinquent groups to other 'constellations of factors which derive from an analysis of the treatment situation itself'. An awareness of 'the psychology of decision taking' in treatment and disposal situations is essential if such needs are to be met. But it equally follows that field workers, psychological or otherwise, should also participate from the start, informed statistical demands being made on them and sophisticated, technical and operational conditions being insisted on by them. Otherwise they will continue to operate on whatever basis is allowed to them by their own technical limitations and the demands of their work situation and statisticians will continue to treat in increasingly complicated fashion material which appears to them to be a blind alley, meaningless psychologically and leading to no effective outcome in social action.

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COMMENTARY

Mr. Wilkins writes: Unless I seriously misunderstand Mr. Morrison, and I do not think that I do, he is not saying that prediction methods are not sufficiently advanced to be used in the field of criminology, but that the disciplines of criminology, penology and jurisprudence are not so sufficiently advanced as to be able to express their hypotheses in scientific terms. They still wish to talk of 'intangibles' and to express other articles of faith which cannot either be proved or disproved. Mr. Morrison wishes to seek perfect knowledge, whilst we know that scientific knowledge must always be partial—this is axiomatic in the terms in which I prefer to think since a complete answer could alone be derived from an information matrix which had no boundary, extending from infinity to infinity, and hence could never be handled. We can only progress piecemeal—from step to step—we cannot reach far from the beginning and we certainly cannot hope to reach the

end. We should, therefore, not design our methods with the object of achieving what we know to be impossible!

But I have not been trained as a case-worker nor in psychology. I took the liberty, therefore, of passing Mr. Morrison's paper to Mr. G. W. Lynch, who has made a special study of criminology and who assisted with the case history material in the Borstal Study. It appeared likely that one who had experience of working with a 'predictionist' and who had case work experience also might be able to resolve the different points of view more readily than could be achieved by direct exchange.

Mr. G. W. Lynch writes: Mr. Morrison's very informative paper interested me particularly in regard to his opinions concerning the usefulness of current predictive methods to workers engaged in 'the diagnosis, treatment or training of delinquents'. The field worker is, of course, guided by the principle of rehabilitating the offender—whose complex structure has to be understood—to a no less complicated social environment in which, or in spite of which, he will not again offend. Are offenders correctly allocated to those forms of treatment most likely to succeed in modifying their psychosocial deficiencies? How are these media of success capable of modification to improve the success rates? To what extent is the criterion of success understood? Answers to questions of this nature offer the means by which field workers can examine the effects of their decisions.

The scientific study of criminal phenomena is still mainly descriptive and consequently it is extremely difficult to validate—or, indeed, to attempt to evaluate—the many postulates which seek to explain causes and effects. It is therefore not surprising that a technology is practically non-existent, or that field workers varying in disciplines, and opinions within disciplines, differ in beliefs and methods. Generally, the field worker has to depend upon subjective judgments by which he may also select any tests or techniques he thinks suitable for application.

If this description approximates to the situation in the field, can there be much doubt about the need to utilize data of an objective nature? At this stage there may be disagreement about what constitutes objectivity, but it would be so much more difficult to sustain disapproval of the goal itself. However, Mr. Morrison does not appear to be fully convinced of this. For instance, he states: '. . . The field worker is not alone in being easily tempted into accusing the statistician of proving the obvious.' Now I would submit that, in terms of practical knowledge in this field, there is very little indeed that could be described as 'obvious'. We cannot yet recognize those who are offenders and those who are not. We deal only with those who are caught, and yet enough is known of a wider circle of

offenders to indicate the hazards which may be involved in developing general theory from a limited and biased class. I make the point to illustrate how uncertain we are at a most rudimentary level in our field of study and to suggest that as a consequence, objective and validated criteria should not be overlooked or too lightly dismissed.

Mr. Morrison also appears to be somewhat preoccupied with the significance of 'intangible' factors. He quotes: 'Concepts which seem to be most immediately amenable to operational formulation and quantitative expression seem to have less "explanatory" power for the clinician.' He then goes on to refer to Mr. Rose and says of him: 'He finally comes to regard data which merely record external events or individual actions as of limited value, and even misleading when compared with the attitudes and emotions operating underneath.' These appear to be statements of preferences on the parts of the authors—convictions, as it were, about the greater significance of non-scientifically demonstrated factors, said to be associated. It is safe to say that 'intangibles' confront scientists in every field and the importance of these 'challenges to science' can hardly be overstated. However, a dilemma appears if a scientific approach—and this, I hope, is part of the connotation of clinician—rejects what has been scientifically demonstrated for that which is at the same time incapable of formulation. Such a conflict of attitudes is responsible, I think, for Mr. Rose's opinion that objective data are misleading when 'compared' with deep-seated emotions. It would be of great interest to know by what criteria he makes his 'comparison' with such conviction.

I cannot understand how these attitudes are likely to lead to progressive development in a technology which is so clearly needed by the field worker. In the present stage where most of us are too often remote from effective scientific explanation, it is, of course, a simple matter to understand why field workers are inclined to look to intuition for answers to their many problems. But, convinced as the individual worker might be about the accuracy of his judgments, the Borstal Prediction Study has demonstrated that certain objective data operated by simple methods can produce a higher degree of accuracy regarding the prognoses of offenders.

This, surely, in itself is enough to commend the study to the attention of all who make decisions in the field. If, in time, workers in the field and in research are led to seek and obtain refinements in data, then, no doubt, the statistician can as readily accommodate those factors in modified tables. At most, it is a scientific process capable of making important contributions towards a standard body of empirical knowledge, and, at least, this one work has shown a method that all field workers can utilize to bring about this discipline.

PROGNOSIS OR PREDICTION : A PSYCHIATRIC EXAMINATION OF THE CONCEPT OF 'RECIDIVISM'

By EDWARD GLOVER (LONDON)

It is indicative of the current trend of criminology that the problem of 'recidivism' should have been chosen as the focal point of discussion at the forthcoming 3rd International Congress on Criminology in London. The study of recidivism, and of the methods of prediction which serve the preliminary purposes of this study, is unquestionably of great importance to criminology, may indeed prove to be of incalculable value in the field of prevention. For if methods of prediction can be as freely applied in the search for 'pre-delinquents' (under which caption we may also include 'larval', 'potential', 'occult', 'latent' or 'predisposed' cases) as apparently they can be in estimating the future conduct of proven delinquents, there is no reason why preventive effort should not be applied where it is most needed and has most chance of success, namely, during that period in the development of children when they become predisposed to crime. At the very least such a course would check some of the improvident expenditure of time, money and energy on undirected or misdirected efforts at prevention which, more often than not, are based on simple preconceptions or even guesses at the probable causes of crime. Research on the subject would at any rate pay for itself.

Now if our interest in recidivism were confined to its descriptive and social aspects there would be no great need to examine the concept too closely. We could accept it as a penological term of limited sociological utility, intended to suggest that some criminal tendencies are more obstinate than others or, more dispassionately perhaps, that some cases continue to offend in spite of the threats or interventions of the law. As soon, however, as predictive methods are exploited to govern *disposal* of potential recidivists we are under obligation to scrutinize closely the exact connotation of the term. And this in turn involves examination of the criteria adopted respectively by psychiatrists, psychologists, sociologists and penologists, not to

mention the standpoint of the statistician. The slightest consideration of the subject will show that, although representatives of these different disciplines may combine their energies and merge their principles on a number of criminological occasions, either diagnostic, therapeutic or preventative, the use of the term 'recidivism' is calculated to bring out essential differences in approach which have become somewhat obscured by the recent expansion of multi-disciplined (team) research.

This is by no means a regrettable contingency; for it serves no useful purpose to gloss over the fact that in certain respects the approaches of, for example, the psychiatrist and the penologist are poles apart. Granted that in some instances they may render common service to the needs of the community for protection or of the individual offender for rehabilitation, yet on such matters as punishment or deterrence differences are bound to arise between them. Indeed, before considering the purely psychiatric aspects of recidivism, it is well to be clear as to the differences between the approach on the one hand of the forensic sociologist, social psychologist and psychologist and on the other of the forensic psychiatrist; also to ascertain what their respective positions are in relation to penology.

In the first place, *the psychiatrist, as psychiatrist, is bound by the professional traditions of his speciality to confine his activities to the cure or amelioration of disordered mental states.* True, there are some few psychiatrists who are prepared to maintain the virtues in some criminal cases of punishment, more rarely of corporal punishment. When they do so, however, they are overstepping their professional boundaries, and in any case their utterances, unless based on convincing researches, have no more validity than the opinions of the penologist or, for the matter of that, of the common citizen. Obviously, therefore, the first step in examining the concept of recidivism is to discover whether it is adapted to the diagnostic, prognostic and therapeutic usages of psychiatry or whether it is fundamentally a penological concept having a moral cast and an implicit social bias.

If we are to judge from court practice in this country it is not hard to establish the implicit moral connotation and social bias of the term. In the eyes of the law the recidivist is regarded as in some degree a 'hardened' criminal on the way to becoming an 'incurable rogue', who cannot be spared the more severe penal and deterrent disciplines at the disposal of the court. Psychiatrists who take up work at delinquency clinics are soon made aware of this fact. Cases which have been put on probation on condition of psychiatric treatment but which, owing to the compulsive or otherwise disordered nature of their anti-social symptoms, are likely to repeat the offence, either in the early stages of treatment or during any intercurrent emotional crisis, are liable on their reappearance in court to be denied further

opportunity of ambulant treatment and despatched forthwith to institutions which, however labelled and however staffed, are essentially penal institutions. And one more 'failure' is recorded in that particular court to the discredit of forensic psychiatry. Yet, if we assume for the purposes of argument that the case has been well selected by the psychiatrist in the first instance, this increase in sentence merely substitutes retributive punishment for incomplete treatment.

To be sure, the tendency to get angry with refractory cases and to react to them as if they were wicked is not confined to the judiciary. It is not unknown in the fields of organic or psychological medicine. But as a rule the organic physician and the psychiatrist would, if pressed, admit that these reactions are subjective and retributive. As far as is humanly possible they try to control or eliminate them; and in any case they do not seek to justify them on the grounds of social expediency. No such restraint need be recognized or exercised by the penologist who, without self-questioning, can plead social expediency in extenuation of his appeal to punishment and discipline.

This professional bias on the part of the psychiatrist permits a ready distinction between his approach to delinquency and that of his co-workers, the social psychologist, the sociologist and the general psychologist. For we need not on this occasion concern ourselves too much with the position of the statistician, who is simply a maid of all work applying his mathematical techniques and statistical controls without any obligation whatsoever to consider the individual or social issues which underlie the data he examines.

To begin with the social psychologist: it is clear that he has a much broader and of necessity more courageous approach to the problem of crime than has the pure psychiatrist. This is no doubt due in part to his lack of intimate contact with the individual offender, to say nothing of his lack of equipment with which to approach him. But in the main it is due to the fact that it is not incumbent on the social psychologist to accept the professional standards, aims and ethics of the psychiatrist. When he approaches the subject of crime he is compelled to approach it as a whole. He need not, indeed cannot, take refuge, as the psychiatrist usually does, behind the unchallengeable position that breaches of the law due to mental or physical disorder should be treated rather than punished. When he makes suggestions regarding the causes of crime or the classification and disposal of criminal offenders, these must be such as apply to the criminal population as a whole. No doubt if the social psychologist were in the habit of giving evidence in court he would find even more difficulty than does the psychiatrist in influencing the bench. Fortunately for him, he is not called upon to do so and exercises his influence on penal practice largely through the prestige he

has gained in administrative circles. As far as punishment and deterrence are concerned, his views, unless based on controlled investigation, have no more prescriptive authority than those of the man in the street. Nevertheless, not being bound by medical ethics, he is free to support penal measures on the score of social necessity.

The sociologist, by virtue of his training, technique and angle of approach, has even less contact with the pathological aspects of individual crime, and his conclusions, like those of the social psychologist, are consequently applicable to crime in general. A student of community or group life, such ameliorative urges as he may possess are motivated primarily by concern for the stability of the group. He, too, is free to support penological measures on the score of social necessity. That in many cases he refrains from so doing is in fact a tribute to his common humanity rather than to his scientific predilections.

The general psychologist is in slightly different case. He does possess techniques of approach to individual crime (mostly, it is true, of a descriptive, characterological type), and in recent years has been increasingly ready to use them in the interests of the individual offender. On the other hand, he has not the overriding interest in and understanding of *pathology* which governs the approach of the psychiatrist. As a general psychologist, he is under no obligation to distinguish 'pathological offenders' from types in which no gross abnormality can be detected. When he does so, as in the application of various personality tests, he is simply acting, whether he knows it or not, as a lay psychiatric assistant. Otherwise his approach is of necessity a general one, without discrimination of disorder, and his conclusions and recommendations, like those of the sociologist and social psychologist, must be applicable to the criminal population as a whole, that is to say, without benefit of psychiatry. He too is free to support the use of penal measures although in fact, like the forensic sociologist and social psychologist, he seldom does so.

It is essential to emphasize this freedom of the social psychiatrist, sociologist and general psychiatrist from professional restrictions, because it accounts for the fact that the term 'recidivism' is less obnoxious to these specialists than it is to the psychiatrist, whose approach to delinquency is governed throughout by concepts of disordered or pathological function, and whose primary interest is in the cure of the pathological offender. It accounts also for the fact that their standing with administrative authorities is higher than that of the psychiatrist. It is indeed more than probable that in the long run the sociologist and social psychologist will do more than the psychiatrist to bring about enlightened measures of disposal applicable to the criminal population as a whole.

Turning now to the history of forensic psychiatry, we find what at first sight appears to be a confusing course of events. For it would appear that, prison medical officers apart, the pioneers in forensic psychiatry were more concerned with the motivation of crime as a whole than with the purely pathological type of persistent offender. This was due in part to the influence of psycho-analytical thinking. During the past thirty years or so psychiatrists have been more influenced by psycho-analytical concepts and principles than most of them would be prepared to admit. And psycho-analytical views on crime were not based on first-hand studies of criminals but were formed at second-hand by sporadic study of such criminal tendencies as might be observed during the analysis of non-criminal disorders. Moreover, psycho-analysts, being specially interested in the influence of unconscious mental mechanisms on human behaviour, tended, and still tend, to apply their principles to *any* form of persistent criminal behaviour, not just to those cases where manifest mental or physical disorder can be established. This explains why court reports presented by psycho-analysts tend to be discounted by magistrates, who are neither persuaded of the existence of unconscious mechanisms nor prepared to accept them as extenuating factors in all and sundry cases of criminal conduct.

Despite this indirect psycho-analytical influence, and no doubt goaded also by long and bitter experience as an expert witness, *the forensic psychiatrist has tended in recent years to confine his diagnostic and therapeutic energies to cases where definite evidence of the existence of pathological factors in criminal conduct can be established.* He has in fact, and in the face of strong opposition, worked to amplify the doctrine regarding criminal responsibility which exists in larval form in the M'Naghten Rules, and to extend its reference from cases of murder to all pathological crime. In this respect his position is unchallengeable and there can be no doubt that in this particular field his authority and influence will increase, except possibly in the case of psychopathy. In the meantime it is sufficient to note that the majority of forensic psychiatrists are reluctant to advocate the application of their findings to anything outside 'pathological crime'. Within that strictly limited field they are concerned with the diagnosis and prognosis of disorders which, although as yet not too well defined, have this in common: that they give rise to anti-social conduct. These conditions forensic psychiatrists propose and, given the opportunity, attempt to treat to the best of their ability.

All of which leads to the main point of this argument, namely, that *to the forensic psychiatrist the term 'recidivism' and, to the extent that prediction is concerned with the forecasting of recidivism, the term 'prediction' are, with one possible exception, redundant, misleading and tendentious.*

To start with the exception : the psychiatrist might be prepared to accept the term 'recidivism' as a description of pathological cases in which the disorder is *episodic*, *intermittent* or *relapsing* in nature, in the same way as he accepts the cycles and intermissions of manic-depressive insanity or the repetitions of obsessional neurosis. Even so he is chary of making clinical use of a term which has so many social and moral connotations and which tends in court practice to bring about the abrogation of what he considers to be the treatment of election, namely, psychiatric treatment of one form or another.

Excluding for the moment the consideration of such episodic cases, the situation can be simply stated as follows. There is no more reason to describe the persistence of symptoms of pathological delinquency as recidivism than to say that tubercular symptoms which persist beyond the average time range or in spite of various measures of treatment are signs of recidivism. It would be equally absurd to describe the victim of a skin disease as a recidivist because his rash continued after tentative steps had been taken to control it. If the diagnosis has been accurate enough and a careful prognosis arrived at, the physician should be in a position to indicate with rough accuracy what the duration of the disorder will be, *e.g.*, whether it will heal spontaneously or by first intention, whether it will require treatment, how long that treatment should last and whether it will end in cure, or some degree of improvement, or leave the patient *in statu quo*. He has no need of a special term to describe chronic or terminal phases. If on the other hand the case is likely to be intermittent or episodic, he should again be in a position to say so; indeed, in such instances it is incumbent on him to warn both the patient and those responsible for him of the intermittent nature of the disorder and to insist that the proper treatment should be continued in spite of or even precisely because of what might appear to be 'relapses'.

To be sure, he recognizes the right of the community to be protected against the depredations of the intermittent or chronic delinquent, but he is entitled to claim that the alleged 'relapsing' case should not be *punished* for his pathological relapses and that the degree of supervision necessitated by considerations of social safety should not exceed that necessary to secure social safety and should permit the continuance of full measures of treatment. In place of the term 'prediction' he would therefore substitute the clinical concept of 'prognosis.'

To the argument that there is no essential difference between prognosis and prediction it can only be replied that the *essential purpose of prognosis is to guide the course of treatment from the outset*. And it would be against all medical ethics to allow treatment to be suspended because of the prospect of failure. It cannot be said that the sole object of predictive techniques

in criminological work is the regulation of treatment. The object of predictive techniques varies with the standpoint of those who have recourse to them. In the case of the statistician no discernible object save that of satisfying curiosity need exist. And at this stage of affairs it would scarcely be fair to expect the penologist to forego any penological advantages that might be secured by the use of prediction techniques.

But, even if we were to waive all terminological considerations, the fact that predictive methods are most commonly employed to estimate the chances of 'recidivism' is liable to give rise to all sorts of misconceptions and tendentious conclusions and to strengthen many existing preconceptions regarding the disposal of offenders. For example, it seems to be generally assumed by the penologist, the judiciary and the public that because an offender has been given a prison sentence, or for the matter of that a suspended sentence (for a suspended sentence is none the less a form of penological influence or, in simpler terms, a threat), he really should not commit the same or any other crime again. In the same way it is generally assumed by courts that, if an offender repeats his offence during treatment, either the treatment has failed or the case is a hardened one. Neither of these propositions will bear close examination. To the contrary, the forensic psychiatrist maintains that in case of 'pathological crime' recidivism is not to be regarded simply as a special manifestation indicative of the failure of remedial, preventive or punitive measures nor simply as an after-history caption, but as *an intrinsic part or natural phase of the disease*. Even so, it is useless to talk of recidivism in cases of pathological crime until all avenues of remedial approach have been fully explored. In any case, it would be more appropriate to use the simple clinical term 'chronic' than to burke conclusions and beg unanswered questions by describing the offender as a recidivist.

To all this the sociologist may well reply that the use of the term cannot be fully argued on the case of pathological crime alone, that pathological criminals represent only a small proportion of the criminal population, and that therefore recidivism remains a pressing problem which cannot be ignored either scientifically or penologically. And the psychiatrist may be compelled to admit, ruefully enough, that the first and last of these contentions are certainly true. But he objects to the second generalization. He can point out what is equally undeniable, that the proper examination of recidivist cases is only in its infancy and that the *statistical boundaries of pathological crime have never been established*. It is true the forensic psychiatrist working at a delinquency clinic deals with a relatively small number of selected cases, and these mostly of a 'favourable' nature. In fact, he only touches a favoured fringe of the subject. On the other hand,

such systematic work as has been done on *unselected* groups of recidivists indicates that a very considerable number of them suffer either from pathological stigmata or have acquired these stigmata through penological mis-handling. Until proper measures of investigation and disposal have been applied to unselected recidivists and the boundaries of pathological delinquency clearly established it is really absurd to draw hard and fast conclusions on the subject.

Moreover, the forensic psychiatrist maintains that it is in the tradition of both psychological and organic medicine to treat the problem of chronicity as a challenge to the resources of diagnosis and treatment, not as an excuse for defeatism or an incentive to the use of penal measures. Cases of so-called 'chronic lunacy' which were once allowed to languish untreated in mental institutions are now given at least the opportunity of reacting favourably to modern methods of physiological therapy. And in an encouraging number of cases a favourable response ensues, proving thereby that what at one time appeared to be the 'hopelessness' of the chronic case was actually a measure of the inadequacy of treatment or, what comes to the same thing, the lack of knowledge of the psychiatrist. From the point of view of research and, in the long run, from the point of view of treatment it is essential to deal with impartial concern with every variety of disorder, acute, sub-acute or chronic, short or long lived, intermittent or continuous, favourable or unfavourable in prognosis, refractory or amenable to treatment; not just to separate the sheep from the goats on grounds of public safety.

Needless to add, the isolation of recidivists in whom pathological factors can be established would not limit the scope of psychiatric approach to the problem of recidivism. Even in cases where no clinical abnormalities can be detected the factor of mental stress is often decisive, and no one is in a better position than the psychiatrist to estimate the importance of this precipitating factor. Without wandering too far from the subject, it may be said that the phrase 'extenuating circumstance' is, like the term 'recidivism', a popular caption which can be better understood by translation into dispassionate terminology and measured in terms of resistance to stress.

Having advanced the psychiatric case for abolition of the term 'recidivism', certainly in cases of 'pathological crime' and possibly in all cases of persistent criminal conduct, it is only fair to add that if this course were followed the responsibilities of the psychiatrist, to say nothing of his sociological co-workers, would be greatly increased thereby. Forensic psychiatry being a comparatively new field of endeavour, it is not surprising that psychiatric classifications of delinquency disorder are woefully inadequate and that they frequently conflict with one another. Nevertheless, the

principles on which classifications can be effected are already clear enough and, if carefully applied, would render superfluous the use of such indiscriminate terms as recidivism in pathological cases. Even if recidivism were synonymous with relapse it would be no more justifiable to apply the term without discrimination than to maintain that all cases of intermittent organic disorder were of the same nature. A recidivist category is essentially a symptomatic grouping, and symptomatic criteria are ill adapted to the purposes of classification. They create more confusion than they eliminate.

Even so, our existing systems of psychiatric diagnosis would go far to reduce the omnibus group of recidivists to some order. If recidivists were examined with the same comprehensive care as is shown in any reputable delinquency clinic, the pathological types would fall easily into well-recognized groups. And each of these could be sub-divided into sub-groups having prognostic standards of their own. To take, for example, the group of so-called 'neurotic' delinquents, it is obvious that the obsessional type is certain to be more chronic than the hysterical type, which tends to be more intermittent and responsive to stress. Even without sub-division (desirable as that is) the psychopathic group could be recognized as in the main 'chronic'. And the same applies to certain forms of sexual offence, *e.g.*, exhibitionism. If in addition some care were taken to estimate respectively the constitutional, developmental and precipitating factors operative in each case, it would not be hard to distinguish those cases which are by their nature chronic from intermittent cases and from cases which are 'recidivist' solely through lack of adequate treatment or through penal mishandling. Needless to add, the therapeutic responsibilities of the psychiatrist would also be greatly increased by psychiatric sifting of the unselected recidivist group; always assuming that the law allowed him the facilities necessary to carry out appropriate treatment of pathological types. The challenge of the refractory case is one that no self-respecting therapist would seek to avoid.

One further consideration. Recidivism, however defined or approached, is not a static problem. Admittedly, in the absence of suitable methods of mass prediction cross-checked by the examination of control groups, it is impossible to do more than hazard a guess as to its course in the next few generations. But it is not unreasonable to suppose that with the isolation and appropriate treatment of the pathological group the figures will dwindle considerably. The residual problem of the non-pathological recidivist must, however, be approached with expectant reserve. It is true that recidivism is to some extent a legacy from the inadequate penologies of the past. And to that extent the application of more sensible penal systems will no doubt assist materially in the reduction of persistent crime, whether pathological

or not. But we cannot expect it to solve the problem. Observers differ as to where the hard core of recidivism really lies, whether it is to be found in what the psychiatrist regards as the pathological group or in the non-pathological group. In view of the fact that the successful (undetected) recidivist merges with the law-abiding and apparently 'normal' population it seems safer to assume that the second of these views is the more accurate. And this bodes ill for the reduction of non-pathological recidivism, unless of course effective methods of prevention can be applied at the pre-delinquent stage.

On the other hand, we may look forward to a reduction in the upper age limit of recidivism. It was one of Freud's greatest services to psychiatry that he established the relation between adult neuroses and child neuroses: 'No adult neurosis without a child neurosis'; and the same correlation has now been established for other disorders, not excluding pathological delinquency. In a sense, therefore, the young adolescent offender can be regarded as already a recidivist, a recidivist to the anti-social mechanisms of his early childhood. It is one of the objections to the use of flat descriptive terms such as recidivism, *l'état dangereux* and the like that they tend to obscure this vital correlation. We have not yet established firmly the dynamic and structural distinctions between the early case, the gradually organizing adolescent case and the adult 'hardened' case. Biased by our need for social safety, we tend to concentrate our attention on the adult type of recidivist and consequently to react to him with a more rigid and disapproving discipline than in the case of young repeaters. With a more elastic attitude and more appropriate measures of prevention and treatment there seems to be no reason why the ceiling of genuine recidivism should not in the long run be reduced to the twenty-five-year mark. But this will involve paying much more attention to the under-seven-year-olds, a field which has the advantage of being free from the interference of the penologist, and one where the psychiatrist, the psychologist, the social psychologist and the sociologist can really meet on common ground. It is in this direction that the predictionist should persistently turn his attention with perhaps a greater appreciation than he has at present of the uses, aims and psychiatric traditions of prognosis.

A FOLLOW-UP STUDY OF CRIMINAL PSYCHOPATHS¹

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In 1948 two of us (D. S.-C. and D. A. P.) undertook the study of a number of criminal psychopaths and ordinary criminals in prison by clinical and EEG methods. The psychopaths were contrasted with a number of ordinary prisoners selected at random but balanced with regard to age and nature of offence. The results were published in this journal in 1951.² The chief object of the clinical study was to establish criteria which would allow the vexed diagnosis of psychopathic personality to be made with greater uniformity and precision. The psychopaths were selected initially by two experienced prison medical officers and were only accepted finally if the diagnosis was fully justified. Subsequent analysis showed that the clinical and historical features which most clearly differentiated the psychopath were, in order of frequency: failure to respond to punishment or to appeals, rebellion against parents, repeated convictions, poor employment record, a lack of plans or of goals, hostility to father, expulsion or truancy from school.

The problem of prognosis, however, is second in importance only to that of diagnosis. A great deal of actual or proposed legislation in the world is based upon the belief that psychopathy is virtually unalterable, at least for many years, although this belief has been based only upon general clinical experience.³ One of us (T. C. N. G.) therefore suggested that he should carry out a follow-up investigation, the results of which are given below. At the request of the Prison Commission, the Criminal Record Office has kindly supplied details of the subsequent convictions of the psychopaths and controls during the five years 1949-53. In some cases the records extend to the middle of 1954.

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² D. Stafford-Clark, D. A. Pond, J. W. Lovett-Doust: *Brit. J. Delinq.*, 1951, 2, 117.

³ T. C. N. Gibbens: *Brit. J. Delinq.*, 1951, 2, 103.

The advantage of this study is that it refers to psychopaths who have been selected upon severe standards. When the original case-sheets were re-examined, however, it was found that in 18 cases the diagnostic formulation contained an element of doubt, *e.g.*, 'Borderline psychopath', 'Not fully fledged', 'Hardly acceptable', etc. Study of their records showed that their behaviour was in fact intermediate between that of the controls and the psychopaths. These 18 'borderline' cases have therefore been removed. The following data refer therefore to 56 controls and 69 psychopaths whose subsequent records are available.

The presentation of criminal records in a systematic way is always difficult. The number of offences has to be shown, but also their gravity as indicated by the sentence. In this case there is a further difficulty that psychopaths frequently have many previous convictions, and the five years in question followed the passage of the Criminal Justice Act, when the new sentences of corrective training and preventive detention were being used freely for persistent offenders. A sentence of 7 years' preventive detention may be given to a persistent offender for an act of housebreaking for which a first or second offender might receive 18 months' imprisonment; length of sentence in this group is related to some extent to length of record and not necessarily to the gravity of the offence, although such long sentences are not given for minor offences. Although the controls were balanced for age and type and severity of the present offence, they could not, of course, be balanced for the number of previous convictions. There were only five controls who had more than four previous convictions, but the psychopaths inevitably had a large number. Seventeen psychopaths had more than eight previous convictions, and only thirteen had less than three.

In the end it was decided to set out the subsequent convictions as shown in Table I, representing both the number of convictions and the length of sentence. The columns from left to right represent a record of no convictions, or of one, two, three and more than three convictions; the lines from top to bottom show whether sentences have never been for more than a year, or whether they have included at least one sentence of 1-2 years, 2-3 years or 4-10 years respectively. For example, an offender with a record of one fine, two sentences of six months, and one of seven years' preventive detention would be placed in the bottom right-hand space as '3 + Convictions, including one of 4-10 years'. He would be similarly classed even though he had also served 3 years' corrective training in addition to these sentences. Fines or probation orders are counted as sentences of less than one year.

In many cases sentences of 2-3 years refer to corrective training: five of the psychopaths had concluded such a sentence and three are still serving.

Five psychopaths in the 4-10-year group have concluded preventive detention, and five more are still serving.

TABLE I

	Nil	1	2	3	3 +	Convictions
Controls = 56	32	8	-	-	2	less than 1 year
		2	2	2	-	1-2 years
		2	3	1	-	2-3 years
		-	1	1	-	4-10 years
	Nil	1	2	3	3 +	Convictions
Total psychopaths = 67	8	10	1	2	1	less than 1 year
		5	4	4	2	1-2 years
		2	4	1	5	2-3 years
		6	8	4	-	4-10 years

It will be seen that the controls behaved according to expectation. It is usually said that some 80 per cent. of first offenders do not return to prison. In this group some 70 per cent. have few or no subsequent convictions. About half were first offenders when seen in 1948, and most of the remainder had up to four previous convictions. It may be added in passing that the clinical descriptions of these controls, though not intended for the assessment of prognosis, proved to be quite closely related to their subsequent history.

The psychopaths have on the whole produced a heavy crop of subsequent convictions, as expected. Nevertheless, the surprising feature is that such a high percentage have *not* been in any serious difficulty. Six of the ten with only one conviction committed quite trivial offences. Approximately 25 per cent. have not been arrested for any serious offence.

There are four possible sources of major error in accepting results of this kind. First, the prisoner might have been serving a long sentence when seen in 1948 and so have had only a brief opportunity of committing offences since his release. In fact, most of them were serving quite short sentences at the time and were released in 1949 or early 1950. There were five cases who have only had 3 years at liberty, and five only had two years. Most of these had offended again. One man had no convictions in only two years' liberty; one aggressive psychopath was released from a 7-year sentence for manslaughter in August, 1953, attempted suicide in October, 1953, and has

no subsequent convictions. This factor thus does not invalidate the general conclusions.

Secondly, there is a possibility that some are dead, especially perhaps by suicide. A search was made at Somerset House for the possible deaths of 41 cases whose whereabouts were doubtful because they had not spent most of the time in prison. One psychopath died during his original sentence and has been deducted from Table I; one other died during a long term of preventive detention.

Thirdly, it seemed probable that a number had become insane and had few or no convictions because they had spent much of the time in mental hospitals. A search was made at the Board of Control for admission to hospital of any of the 41 doubtful cases. One, removed from the total, was sent to Rampton during the original sentence. One was temporarily certified after release from preventive detention, but is now discharged and has not been re-convicted.

Lastly, they may have emigrated. This remains a possibility, but the criminal record of a man who went to Australia is available, so it is possible that crime in the Dominions would have been reported.

These errors have therefore no significant bearing. With this in mind some results are distinctly surprising. For instance, an aggressive psychopath of 39, with eighteen previous convictions, including many charges of grievous bodily harm and assault, was released in January 1951 after serving a three-year sentence for grievous bodily harm. In May 1951 he was fined for a drunken assault; in January 1953 was bound over for gaming; since this there are no subsequent convictions. Again, a man of 37, described as a 'classical inadequate psychopath of cultured type', with fourteen previous convictions for larceny and forgery, was released in 1950 from a two-year sentence for robbery with violence; there are no subsequent convictions.

In the original study the psychopaths were divided, for the purpose especially of EEG study, into psychopaths with epilepsy, those with head injury, and those without these defects. Table II shows that the after-history of these groups was rather similar. There is perhaps a tendency for those with head injury to have a worse prognosis; over 40 per cent., for instance, have three or more convictions as opposed to 20 per cent. or less in the others.

No attempt was made at that time to separate inadequate from aggressive psychopaths or make other clinical divisions. But a brief diagnostic formulation was made in each case which referred to aggressive tendencies, *e.g.*, 'Predominantly aggressive', 'Typical young aggressive psychopath', or 'Very immature and inadequate, with little aggressiveness'. These have been used independently to divide the psychopaths into aggressives and others (predominantly inadequate).

TABLE II

	Nil	1	2	3	3 +	Convictions
Psychopaths with- out head injury or epilepsy = 31	6	3	—	1	—	less than 1 year
		3	1	2	2	1-2 years
		1	2	—	—	2-3 years
		2	6	2	—	4-10 years
	Nil	1	2	3	3 +	Convictions
Psychopaths with head injury = 26	1	5	—	1	1	less than 1 year
		2	1	2	—	1-2 years
		—	2	1	5	2-3 years
		2	2	1	—	4-10 years
	Nil	1	2	3	3 +	Convictions
Psychopaths with epilepsy = 10	1	2	1	—	—	less than 1 year
		—	2	—	—	1-2 years
		1	—	—	—	2-3 years
		2	—	1	—	4-10 years

Table III shows that the subsequent convictions in these two groups are significantly different. The aggressive psychopaths had a worse prognosis from almost all points of view. Only 13 per cent. of the inadequates have three or more convictions as opposed to 50 per cent. of the aggressives ; 35 per cent. have little subsequent recidivism as against only 8 per cent. of the aggressives.

It will be noted that this is in some ways the opposite of what clinical impressions of the inadequate and aggressive psychopath would lead us to expect. The popular conception of the inadequate psychopath is of a man who persists incorrigibly in committing rather minor offences, while the aggressive psychopath commits major aggressive offences at longer intervals. The histories of these men suggested that the opposite was often the case—that the aggressives included some who produced a chain of relatively petty aggressive offences as well as others with serious acquisitive offences.

TABLE III

Inadequate psychopaths = 44	Nil	1	2	3	3 +	Convictions
	7	9	1	1	—	less than 1 year
		4	3	2	—	1-2 years
		2	3	1	2	2-3 years
		4	5	—	—	4-10 years

Aggressive psychopaths = 23	Nil	1	2	3	3 +	Convictions
	1	1	—	1	1	less than 1 year
		1	1	2	2	1-2 years
		—	1	—	3	2-3 years
		2	3	4	—	4-10 years

The Electroencephalographic (EEG) Results. In interpreting the following results it must be remembered that EEG technique has made great strides in the last five years; study by modern methods might have revealed much more valuable information.

TABLE IV—EEG

	<i>Normal</i>	<i>Abnormal</i>	<i>Doubtful</i>	<i>No Record</i>
Controls	38	6	6	13
Borderlines	10	7	1	0
All psychopaths	29	34	10	9

As reported before, there is no doubt about the significance of abnormal EEGs in psychopaths. Table IV shows that in the original sample abnormality was found four times as frequently in the psychopaths as in the controls. It was found, however, that the prognostic significance of an abnormal EEG was rather more doubtful. The six controls with abnormal EEGs were not distinguished in any way from the rest—three had no subsequent convictions. Similarly, psychopaths with an abnormal EEG did not have criminal records which were significantly different from those with normal EEGs.

On the whole EEG abnormalities are found less frequently with advancing age. One might expect, therefore, that the prognostic significance of an abnormal EEG in psychopathy would vary with the age of the subject. There was some evidence that this was so. It was found that, in psychopaths of under 25, abnormality of the EEG was of no particular significance. But in psychopaths over 25 there was a hint, in spite of small numbers, that abnormality of the EEG indicated a more favourable prognosis, as shown in Table V.

TABLE V

		Nil	1	2	3	3 +	Convictions
Psychopaths over 25	Abnormal EEG = 14	3	3	1	—	—	less than 1 year
			1	—	1	1	1-2 years
			—	—	—	—	2-3 years
			2	2	—	—	4-10 years

		Nil	1	2	3	3 +	Convictions
Psychopaths over 25	Normal EEG = 19	1	2	—	1	—	less than 1 year
			2	1	1	—	1-2 years
			1	—	—	2	2-3 years
			3	3	2	—	4-10 years

The EEG in cases with head injury was not definitely related to prognosis. If anything, those with an abnormal EEG had a better prognosis.

When the question of EEG abnormality was considered in the aggressive and inadequate groups, the relation to prognosis appeared more definite. Table VI shows that in the aggressive group abnormality is not noticeably related to prognosis; if anything, abnormality is related to a bad prognosis. In the inadequate group, however, there is rather clearer indication of the opposite relationship—that abnormality of EEG is a good prognostic indication. It is possible that the contrary tendency in the two groups is the reason why, in psychopaths as a whole, EEG abnormality is not related to prognosis.

TABLE VI

	Nil	1	2	3	3 +	Convictions
Aggressive psychopaths with normal EEG = 9	1	1	-	-	-	less than 1 year
		-	-	1	-	1-2 years
		-	-	-	2	2-3 years
		1	1	2	-	4-10 years
	Nil	1	2	3	3 +	Convictions
Aggressive psychopaths with abnormal EEG = 13	-	-	1	-	1	less than 1 year
		-	2	-	3	1-2 years
		-	1	-	1	2-3 years
		1	2	1	-	4-10 years
	Nil	1	2	3	3 +	Convictions
Inadequate psychopaths with normal EEG = 20	2	2	-	1	-	less than 1 year
		3	1	-	2	1-2 years
		2	1	-	2	2-3 years
		2	2	-	-	4-10 years
	Nil	1	2	3	3 +	Convictions
Inadequate psychopaths with abnormal EEG = 13	4	4	-	-	-	less than 1 year
		-	1	-	-	1-2 years
		-	1	-	-	2-3 years
		1	2	-	-	4-10 years

Aggressive Crimes. Since it appears that aggressiveness is the most important feature of the psychopath, further consideration was given to the nature of the aggressive offences they committed. Attempted suicide was much less common than expected. Two aggressive psychopaths and two

controls subsequently attempted suicide, one of them in association with a major crime. The most interesting feature of the subsequent criminal histories of these psychopaths was that they committed relatively few aggressive crimes. Twenty-four aggressive psychopaths whose records are available committed 72 offences in 5 years, but only 14 of these were purely aggressive offences and only 3 of them serious enough to be punished by imprisonment. This does not include four cases of indecent assault in which the element of violence is very variable. It is equally true of course that apparently acquisitive offences, such as attempted shopbreaking, sometimes conceal a crime which is potentially dangerous and aggressive, *e.g.*, a gang attempting a smash-and-grab raid in a car.

On the other hand, the aggressive offences committed by the inadequates (though only half as frequent) were sometimes serious. The most violent offences are, of course, commonly committed by individuals who cannot normally express their aggressiveness.

The aggressive offences subsequently committed by the three groups of aggressive psychopaths, inadequate psychopaths and controls were as follows:

(1) *Aggressives*. 7 out of 24 individuals committed aggressive offences, comprising: 11 fines for assaulting police, wilful damage, etc.; one case of 6 months' imprisonment for assaulting the police; one of 15 months for grievous bodily harm; one of 3 years for grievous bodily harm.

(2) *Inadequates*. 6 of 45 individuals committed aggressive offences, comprising three sentences of less than 2 months for assault, one of 6 months for assault, two of 12 months for assault; one of 4 years for manslaughter; one of 7 years for robbery and larceny. There was one case of indecent assault.

(3) *The Controls*. 3 of 63 individuals committed aggressive offences, comprising 11 fines for assault, wilful damage, etc.; one sentence of 2 months and one of 6 months for assault of the police.

The offences for which these psychopaths were originally convicted at the time of their examination were often aggressive; twenty psychopaths were sentenced for such offences, and nine of the 63 controls. Among the controls were five cases of robbery with violence. The subsequent record of one is not available, one has committed a minor offence and the three others have no subsequent convictions in 5 years.

Aggressive crime presents a complex problem. It seems probable that the persistent aggressiveness of the psychopath is so crippling to his social relations that he is only able to live by crime, and his record thus tends to be composed of serious acquisitive offences. A typical severe record reads as follows: Aggressive psychopath aged 18½ with abnormal EEG; five

previous convictions; present offence, robbery with violence and assaulting a Borstal officer with a knife. Subsequent record: 1949, 18 months; 1951, 6 months, 21 months; 1952, 6 months; 1953, 6 months—all for housebreaking or larceny; 1954, placed on probation for a letter threatening to kill.

On the other hand, everyone who sees prisoners regularly knows that criminal records almost entirely composed of aggressive offences, sometimes fairly serious ones, are by no means uncommon. The majority of such cases are probably not psychopaths; the record itself suggests that they are capable of social adjustment and that their aggressiveness is localized, although it may be a serious disability. One such case, for instance, seen by one of us (T. C. N. G.) during a study of cruelty to children had a record as follows: 1933, larceny (probation); 1940, shopbreaking (probation); 1941, larceny (£3 fine); 1943, assault police, wilful damage (2 months' imprisonment and fines); 1945, assault occasioning actual bodily harm (6 months); 1946, grievous bodily harm (9 months); 1949, assault, actual bodily harm (6 and 6 months consecutive); 1951, assault occasioning bodily harm (15 months); as well as ten summary convictions for drunkenness, etc.; EEG abnormal. This man was described as a good and regular worker when not in prison, and was devoted to his wife and children in spite of many quarrels. He could not be called an aggressive psychopath.

We should bear in mind the apparent paradox that the persistently aggressive offender is often not a psychopath, and that the aggressive psychopath often does not have a particularly aggressive record on paper.

So far as clinical types are concerned, there appeared to be two interesting groups. First there was a group of 8 cases who had a large number of offences in 5 years. These were mainly between the ages of 23–26, 7 of the 8 were aggressive psychopaths, and they had five, six or seven convictions of intermediate severity in the five years. Six had a past head injury, three were described as very immature. The EEGs were half normal, half abnormal. One example has been given. It may be that they are typical of the fully-fledged aggressive psychopath at any age when he has not yet qualified for preventive detention but is too incorrigible to be suitable for corrective training. Though these offences are relatively minor ones, they are not like those of the persistent petty offender with a long history of fines for drunkenness or a week or two's imprisonment for petty theft.

Secondly, there is a group which were described in the original paper as 'Charming inadequate psychopaths', incorrigible but cultured, polite and charming in manner. Seven fell into this group. They are inadequate perhaps only in contrast to the aggressive group, some who perpetrate skilful frauds being far from inadequate. Six of them were without epilepsy or

head injury and they tended to concentrate on fraud and false pretences. The EEGs of four were abnormal; three were normal. The after-history of this group was perplexing. Four were very similar in having two convictions, one of one year or so, and one of three years. Of the rest, two had no subsequent convictions, and the third had one very minor offence. On the whole this type of psychopath is better educated; for a time they may find family support or a job well within their limits.

Summary and Discussion. The criminal records of 69 prisoners, carefully selected in 1948 as examples of severe psychopathic personality, have been studied for the years 1948-53, as well as those of 56 controls. In doubtful cases a check was made at Somerset House and the Board of Control to establish that they were not dead or in mental hospitals.

On the whole the psychopaths showed a high rate of crime, as expected, but as many as a quarter had very few or no subsequent convictions. Those diagnosed as aggressive psychopaths, however, had a definitely poor prognosis. Inadequate psychopaths, if young and with abnormal EEGs, had such a variable career that the diagnosis should be made with great caution if it is meant to imply a poor prognosis. The term 'psychopath' should either be restricted to seriously aggressive cases or it should be borne in mind that spontaneous recovery or a good response to treatment is quite possible so far as criminal tendencies are concerned.

EEG abnormality was not related to the after-conduct of psychopaths as a whole. A similar finding was that of Pond and Landucci¹ on behaviour problem children in whom the prognosis was the same whether the EEG was abnormal or not. There was some evidence, however, that in all psychopaths of over 25, and in inadequate psychopaths, abnormality of EEG is a favourable prognostic sign. It is logical to suppose that abnormality of EEG indicates the possibility of change by maturation. It is probably more difficult to differentiate the older psychopaths, especially if inadequate and with normal EEG, from the ordinary chronic recidivist whose criminal habits are ingrained and perhaps less likely to alter.

Although aggressive psychopaths are very likely to offend again, their offences are not necessarily serious, and are more likely to be acquisitive than aggressive, probably because their aggressive tendencies cause a complete social breakdown and incline them to acquisitive crime. The aggressive psychopaths committed 74 offences in five years, 14 being aggressive offences, but only three of these were serious. There is no doubt that they are often very serious and inveterate offenders, but it does not appear justifiable to regard them as, for example, potential murderers.

¹ L. Landucci and D. A. Pond: *Minerva med (Torino)*, 1954, **1**, No. 7.

RESEARCH AND METHODOLOGY

FOLLOW-UP ENQUIRY ON RESULTS OF RESEARCH SCHEMES

Since the appearance of its first number, this Journal has published brief accounts of current research into various aspects of crime and delinquency ; the Editorial Board has now carried out a follow-up enquiry into the progress of these schemes. Further information on some of them has been summarised below, and details of the rest will follow in a later number.

The survey of prostitution by Mrs. Rosalind Wilkins (*Current Research*, I, Vol. I, p. 62) has led to the publication of a book, 'Women of the Streets', Ed. C. H. Rolph (Secker and Warburg, 1955).

An article discussing the methodological aspects of Dr. T. Grygier's enquiry on the leisure pursuits of juvenile delinquents (*C.R.* II, Vol. I, p. 63) appeared in the *Brit. J. Delinq.* Vol. V, No. 3 of January, 1955 ; a monograph giving the results of his study will eventually be published.

Dr. W. Warren's investigation of conduct disorders in children aged 5 to 15 years (*C.R.* III, Vol. I, p. 63) resulted in an article in the *Brit. J. Delinq.* Vol. I, No. 3, pp. 164-86.

The results of Dr. John Spencer's study of service life and criminal behaviour (*C.R.* IV, Vol. I, p. 134) have been published in a book, 'Crime and the Services' (Routledge and Kegan Paul, London, 1954), and Dr. Howard Jones's study of group methods in the institutional treatment of children (*C.R.* V, Vol. I, p. 134) was reported in a Ph.D. thesis (London, 1953), but has not yet been otherwise published.

An article by Dr. Brian Kay, describing the reactions of delinquents and other groups to experimentally induced frustration (*C.R.* VI, Vol. I, pp. 134-5) appeared in the *Brit. J. Delinq.* Vol. IV, No. 4, pp. 245-64. Mr. A. G. Rose's follow-up study of Borstal boys (*C.R.* VII, Vol. I, p. 205) is reported in a book '500 Borstal Boys' (Blackwell, Oxford, 1954).

Dr. Sessions Hodge's investigation of cerebral maturation and aberrant behaviour (*C.R.* VIII, Vol. I, p. 205) revealed a close connection between an 'alphoid' EEG rhythm, colour dominance in visual perception and a tendency to emotional immaturity or disturbance, in a group among whom the delinquency rate was high. The relation of these findings to Rorschach colour and form responses is discussed, as is the possible connection between autonomic lability and the occurrence of alphoid frequencies, in the E.E.G. record.

The results of Miss Mary Tingle's study of the intake at a classifying school for girls (*C.R.* IX, Vol. I, p. 205), are reported in an M.A. Thesis 1954, which is obtainable at the University of London Library. A comparison between admissions in 1944 and 1950 lent little support for the view that war-time delinquents came from a type of home or neighbourhood dissimilar to that of girls admitted after the war, in economic, social or psychological aspects. However, certain psychological differences between the girls themselves were observed : these seemed to indicate that, during the war years, more stable and well-adjusted girls tended to become delinquent than in post-war years.

Mr. J. B. Mays' local study of juvenile delinquents in the neighbourhood of a

University Settlement (C.R. X, Vol. I, pp. 205-6) has led to the publication of a book, 'Growing Up in the City' (University Press of Liverpool, 1954) and to an article in the *Brit. J. Delinq.*, Vol. III, No. I, pp. 5-19).

A paper on *Mr. John Mack's survey of activities concerned with juvenile delinquency* (C.R. XI, Vol. I, pp. 206-8) is to appear in the July 1955 issue of the *Sociological Review* (N. Staffs) under the title of 'Juvenile Delinquency Research—a Criticism.' The results are also discussed in a chapter on 'Crime' in 'The Scottish Economy', 1954, and in the forthcoming Glasgow volume of the 'Third Statistical Account of Scotland.'

The results of *Dr. Alice Buck's and Dr. T. Grygier's study of the psychotherapy of juvenile delinquents* (C.R. XIII, Vol. I, p. 293) have been published as a paper in the *American Journal of Psychotherapy* of October 1952, and *Dr. Peter Scott's investigation of the use made of sections 26 and 4 of the Criminal Justice Act, 1948* (C.R. XV, Vol. II, p. 46) has been published in the *Howard Journal*, Vol. IX, No. I, 1954, pp. 20-29, under the title of 'The Criminal Justice Act of 1948 and the Psychiatrist'; this is also available as a pamphlet, published by the Howard League.

M. WOODWARD.

THE IMPACT OF CORRECTIONAL PROGRAMMES ON INMATES

The assumption in this paper is that inmate persons know what they are getting out of their stay in a penal or correctional institution, know from what part of the total institutional programme they are getting the most benefit, and know what staff member likes them the best, understands them or has helped them the most. The inmate is not necessarily different from ordinary persons, who can tell others what they are getting out of certain experiences. It has been found that individuals can tell quite reliably whether their marriages are happy or unhappy and that their best friends can also tell reliably whether the observed marriage is happy or unhappy. It is true that there will be a minority of persons who give evasive or conventional answers to questions which probe their private lives, thereby masking their real feelings. But there are ways to detect or surmount this shortcoming. The great majority of persons in America will co-operate with a sincere, well-timed, confidential approach which seeks to elicit their feelings about their experiences, situations, or problems.

If we can accept the fact that inmate persons, under proper auspices, in the proper setting, and at the proper time, will reveal their attitudes and opinions about their institutional experiences, we have some measure of the impact of staff and programme upon them. Such soundings will be called impact studies, because they attempt to indicate what is happening to the inmate person as he views himself and his situation. Impact studies, using inmate opinions and attitudes, can become gauges of the operational effectiveness of correctional programmes and methods.

Galway's Study. The writer has been pursuing a series of impact studies, principally through the dissertations of his Ph.D. students. The more important ones will be briefly described in regard to their most pertinent findings. The pilot

study in the series was made by Edward J. Galway in 1947 at the U.S. Reformatory, Chillicothe, Ohio. A standardized terminal interview was held with 275 consecutively released inmates, privately and individually, just before departure. Seventy-two per cent. indicated that the institutional stay had been beneficial to them.¹ After answering the initial question 'Have you got anything out of being here?' in terms of yes or no, the inmate was told he could elaborate and say anything else he wanted to. His comments were recorded. The positive answers were found to fall into four groups from most to least frequent: (1) the socializing and maturing value of his institutional stay; (2) the deterrent effect of being institutionalized; (3) the acquisition of skills; (4) miscellaneous, such as curing alcoholic tendencies, improvement in health, etc. The comments in support of the negative answers most frequently contained statements to the effect that there really was no need to be sent to the reformatory and no need for rehabilitative treatment. Less frequently the negative comments inferred that, while the inmate's attitudes and habits were not socially acceptable, they have not changed as a result of the institutional stay. A small minority of negative replies indicated that the inmate merely 'pulled time' in the institution. Finally, a few inmates said that they had been worsened by the experience.

Galway found that the Negro inmates gave positive replies much more than whites: 89 versus 68 per cent. Inmates who were given an initial custodial classification of 'close' gave negative replies in 38 per cent. of the instances (the over-all being 28), while inmates who received an original custodial classification of 'medium outside' gave negative replies in only 17 per cent. of the instances. Stating the matter reversely, the medium outside inmates were more favourably disposed in their replies than were the close inmates. Favourable and unfavourable responses were not related significantly to disciplinary offences during the stay, although inmates with three or more disciplinary actions gave yes replies to the impact question in only 60 per cent. of the instances (the over-all being 72). Favourable or unfavourable replies were not significantly related to the inmate's previous institutionalization. Neither were the responses related too significantly to the presence or absence of psychiatric disorders (just below the 5 per cent. level of confidence). Favourable and unfavourable responses of inmates were not related to age, but the inmates under 18 were much less favourably disposed than inmates 24 years of age and over. The inmates released on expiration, the ones who evidently are the poorest risks for parole or the most involved in previous crime, displayed only 50 per cent. favourable response, but the numbers were too small to make a significant finding. Subjects with the lowest education level gave much more favourable responses than inmates of higher educational levels, which finding might reflect the opinion of Negroes and the response of least fortunate groups to visibly good opportunities for improvement.

Galway also asked the departing inmates to indicate which staff member knew them best. The 275 inmates nominated 125 different staff members, some, of course, more than one time. These 125 constituted 40 per cent. of the staff on the pay roll at the time. The 275 nominations of staff members who knew the inmate best (in the inmate's judgment) were distributed by branch of the service as follows: custody, 108; trade training, 39; farm, 32; industry, 18; maintenance, 18;

¹ Edward J. Galway: 'A Measurement of the Effectiveness of a Reformatory Program', Ph.D. Dissertation, The Ohio State University, 1948, p. 43.

culinary, 17; classification and parole (social workers), 14; clerical, 13; educational, 9; medical, 5; chaplain, 2. Says Galway: 'It is significant that the personnel which the subjects chose as knowing them best came, by and large, from that portion of the personnel known in less progressive correctional institutions as the guard.'

The evaluations of the nominated officers as to whether the nominating inmate has got anything out of his stay did not correlate with the self-evaluations of the inmates. Galway also found that the officers did not have the understanding or the concepts of behaviour which enabled them to tell whether an inmate had really changed for the better during his stay. On the other hand, Galway found that the inmate nominees (the inmate who knew the departing inmate best) made evaluations of impact more nearly like those the nominating inmates made of themselves. The inmate nominees thought in terms of whether the nominating inmate was prepared to go straight or whether his anti-social attitudes had been modified, whereas the staff members thought in terms of improvement in work habits or skills. Galway contended that professional staff could be wheeled in line to have more positive impact and that their influence could extend to custodial and supervisory and other staff, in terms of developing understanding and awareness of behaviour problems.

Bright's Study. Administering on 1st May, 1950, a carefully-constructed questionnaire to five small samples (fifty each), representing five different groups of inmates in the Ohio Penitentiary at Columbus, Ohio, David Eugene Bright found good evidence to support the following three propositions: '(1) The longer the time served in prison, the more adverse will be the attitudes of the inmates, as indicated by the responses of the inmates to questions concerning the personnel, the programme, and the physical facilities of the prison; (2) the lower-paid, non-professional staff members create more impact than do the higher-paid, professional staff members, as indicated by the inmates' nominations of staff members they like best and of staff members who have done something for them; and (3) better prison programme and facilities lead to better attitudes, as indicated by the responses of inmates to questions concerning the programme and the physical facilities.'²

Bright concluded that 'by getting better personnel at the guard and the work supervision level, better programme, and better physical facilities and by decreasing the time of incarceration, prison administrators can increase the constructive impact of the institution on a large inmate population.'

For the record, the five samples of fifty prisoners each consisted of inmates who had been in the penitentiary three months, inmates who had been in fifteen months, inmates who had been in three years, inmates who had been chosen for transfer to the honour farm, inmates living in the honour dormitory (who share the greatest privileges).

Sabnis' Study. M. S. Sabnis in September 1951 administered a carefully prepared questionnaire to three samples of boys (forty each) at the National Training School for Boys, Washington, D.C. The first sample was representative of boys who had been in the institution about three months; the second, of boys who had been in the institution about six months; the third, of boys who had been

² David Eugene Bright: 'A Study of Institutional Impact Upon Adult Male Prisoners', Ph.D. Dissertation, The Ohio State University, 1951, pp. 84-5.

in the institution eight months or longer. Each sample of forty boys was matched groupwise for age, race and offence distribution to ensure similar stratification. A fourth group, not matched groupwise with the other three samples, was taken from the consecutive releases at approximately this period. These boys were given a terminal interview in accordance with a standard interview schedule, privately and individually, just before departure, as in Galway's study.

Based on what the boys said they got out of their institutional stay, Sabnis attempted to demonstrate four operating hypotheses in correctional work and came to the following conclusions:

(1) 'The first hypothesis that the more sophisticated the inmate is, the more likely he is to profit by institutional experience is not valid in accordance with certain criteria of sophistication, such as educational achievement, urban or rural affiliation, favourable or unfavourable informedness about the institution, and previous institutionalization.'

(2) 'The second hypothesis that impact reaches its optimum positivity at a certain point of time (period of institutionalization), and that thereafter it tends to vary inversely with the length of time put in by the inmate, is validated.' This supports Bright's findings at the Ohio Penitentiary. We will return to the specifics of this conclusion later on.

(3) 'The third hypothesis that it is the non-professional personnel more than the professional personnel who come closest to the inmates, help the most, understand them the best, and generally exert the greatest impact on them is partly validated, inasmuch as it was demonstrated that while the non-professional personnel were characterized by their willingness to come closest to the inmates and helping them the most, their effective functioning was apparently being hindered by their undeveloped awareness of the end of the institutional programme and their insufficient discernment of the dynamics inherent in the reconstruction of the inmates' social behaviour and social attitudes.' Substantiation of this finding has already been made by Galway and Bright.

(4) 'The fourth hypothesis that there are certain programme areas which can have impact and can reach the boys, and these when improved lead to better attitudes and result in relatively better behaviour, is validated.'³

Sabnis concluded that his study 'has demonstrated that the orientation and classification processes (at this school) are sufficiently geared to the inmates' settling down to the serious business of getting as much out of the programme as possible during the first three months of their stay. The findings of the study, however, point up a need to overhaul the educational and recreational programme which could ensure the continuity of constructive impact being exerted on them during the initial period of institutional experience; to augment the social service programme in which frequency of contact by the (professional) personnel could be accelerated; to strengthen the chaplain services which were acknowledged by the subjects as having some beneficial influence on them; to enrich and enliven the cottage life (programme) by improving facilities, providing organized indoor recreation and professional group-work and brightening the cottage atmosphere to approximate as far as possible to home surroundings; to select such personnel to be in charge of cottages, at least during waking hours, as are sensitive to the

³ M. S. Sabnis: 'A Measurement of Impact of Institutional Experience on Inmates of a Training School', Ph.D. Dissertation, The Ohio State University, 1951, pp. 149-50.

concept of re-education and rehabilitation as fundamental to correctional philosophy and can through the opportunities of in-service training develop a unified point of view.'

Some specific findings from the Sabnis study will be hurriedly mentioned. The positive response to the question, 'Do you think you are getting anything out of your stay here?' ranged from 83 to 90 per cent. among the four sample groups. A cumulative favourable response to 23 questionnaire items, covering varying aspects of the total institutional programme, was most evident for the boys who had been there only three months, was lower for the boys who had been there six months, and still lower for the boys who had been there eight months or more (see hypothesis (2) above). From 57.5 to 67.5 per cent. of the boys in the above three samples endorsed the statements: 'The boys I know in here are getting a lot of good out of their stay,' and 'Boys in this school really change for the better while here.' Contrarywise, around 45 per cent. of the boys endorsed the statement that 'a boy at this school is made worse by other boys here.' Still around the same point, about 85 per cent. felt that most boys learn to manage their life better while at the school. From 78 to 93 per cent. of the boys professed that they noticed real improvement in themselves since they have been at the school.

Finally, certain specific findings from the release group, via the terminal interview, are of interest. Only 10 per cent. said that they had just put in time during their stay. Eighty per cent. said they had changed during their stay. In regard to what part of the programme the releasees got the most from, only 2.5, 2.5, 5 and 7.5 per cent. respectively designated group therapy, health service, chaplain, and cottage life as their first nomination. Caseworkers, psychologists, recreation leaders were not nominated by the releasees. Eighty per cent. indicated that the experience in the school would help them on the outside.

Chillicothe Revisited. Incidental to Moran's study of the inmate's concept of self, reported on later, the writer had certain impact items covered in terminal interviews with 250 inmates consecutively released from the U.S. Reformatory at Chillicothe, Ohio, exactly six years after the original Galway study. Sixty-eight per cent. professed to have got a good deal out of their stay; 23 per cent., something; 9 per cent., not much. The above distribution of answers was significantly related to professed favourable or unfavourable feelings toward staff members. This suggests that inmates will feel that they have obtained some benefit from the programme of an institution, if at the same time they feel favourably disposed toward staff. In response to the terminal interview question, 'Do you think you have changed at all during your stay?'—structured for three answers—73.2 per cent. indicated a lot; 20.4, some; 6.4, very little.

Supplementing this question, the inmate was asked to state: 'In what way do you think you have changed?' The answers were classified into the following categories: learned lesson, 69 instances; more settled, 43; attitude improved, 35; grown up, 24; matured, 24; think before acting, 21; assume responsibility, 13; control temper, 9; work habits improved, 8; no answer, 4. It is interesting to note that practically all these answers would come under the heading of maturation.

'Do you think the fellows here make a fellow worse?' This question elicited the following responses: 34 per cent. said no; 21.6, some; 28.4, depends on individual; 15.2, yes; less than 1 per cent., no answer. 'Do any fellows or groups of fellows make it tough on a fellow while here?' The answers: 57.2 per cent. said

a few do; 37.2, some do; 5.2, a great many do; less than 1 per cent., no answer. From these several angles it looks as if the reformatory at Chillicothe is having a favourable impact on inmates as they view the situation at the time of their departure. The responses seem to be in line with those obtained in other studies.

'Name the staff member that helped you the most. What was his branch of service?' The nominations were distributed as follows: 37.6 per cent. fell into the custodial branch (housing, maintenance, supervisory); 19.2, non-custodial maintenance; 12.8, industry foremen; 11.6, trade training supervisors; 17.6, professional (social workers, academic teachers, doctors, etc.); 1.2 per cent., no answer. This distribution still conforms pretty much to the findings of the previous studies of the earlier Galway study, although there is a greater proportion selection of professional staff members in the more recent Chillicothe sounding.

'What part of the programme did you get the most out of?' The first-choice nominations were distributed as follows: work assignment, 112 instances; trade training, 45; recreation and athletics, 28; school, 20; religious, 16; library, 15; cell-house or dormitory life, 8; health, 4; food, 1; no answer, 1. This distribution of answers conforms pretty much to the one which Sabnis found for his forty releasees at the National Training School for Boys: trade training, 13 instances; recreation, 8; academic, 5; chaplain, 2; cottage, 3; food, 3; group therapy, 1; health, 1; work, 4.⁴

Zibners' Study. One hundred boys and girls consecutively released from the Bureau of Juvenile Research of Columbus, Ohio, in the last quarter of 1953 were studied by Harry Zibners, in order to estimate the impact of their short residential stay at this diagnostic centre. Only the children 13 years of age and over were given a standard terminal interview at the time of discharge. Eighty of the 100 children had been committed to the centre by juvenile courts of Ohio; 20 were referrals from agencies.⁵ The principal worker at the centre, according to present staff structure, is the clinical psychologist, who is known as the child's 'worker'. The psychologist sees a lot of the child during his residential stay.

We have reported on an impact study at the penitentiary level, at the young man's reformatory level, and at the training-school level for delinquent youth. Here we are reporting on an impact study at a still younger level—about as young in age as is possible to go.

Zibners made impact soundings on seven items: 1. Do you think it good for you to be at the Bureau for some time or do you think it was not good for you? 2. Do you think that during your stay at the Bureau you learned to understand better why you got into trouble? 3. Do you think your stay at the Bureau will help you to get along better with other people on the outside? 4. Do you think the other kids at the Bureau made you worse or do you think they helped you to become better? 5. Would you say that the people at the Bureau really tried to help you? 6. Do you think that most of the other boys/girls are getting anything out of their stay at the Bureau? 7. What chance do you think you have of staying out of trouble in the future?

The response of the children to these impact questions was very highly favour-

⁴ Sabnis, *op. cit.*, pp. 108-9.

⁵ Harry Zibners: 'The Influence of Short-Term Institutionalization Upon Emotionally Disturbed and Delinquent Children', M.A. Thesis, The Ohio State University, 1954, pp. 33-40.

able, much more so than in any of the other impact studies. Zibners arranged for the psychologist who had been assigned to each child during his stay to give an independent rating on what he got out of his stay at the Bureau, soon after the child's discharge. Likewise he got ratings from a staff person in the cottages whom each child nominated as knowing him best. These adult worker responses on comparable items were significantly lower than the children's responses, which fact led Zibners to conclude that the children over-estimated the positive impact of the residential centre. He also found that girls over-estimated the positive impact more than boys, while older boys over-estimated more than younger boys. White boys of higher I.Q. levels (above 96) tended to over-estimate favourable impact more than white boys of lower I.Q. levels (95 and under). Favourability of response was not related to race, length of stay, AWOL experience, or previous court experience.

Two supplementary questions were asked the children: 'Who do you think helped you the most?' 'Who else?' The first yielded the following answers: psychologist, 34 per cent.; teacher, 4 per cent.; cottage parent or recreation worker, 62 per cent. The second yielded the following: psychologist, 17 per cent.; teacher, 5 per cent.; cottage parent or recreation worker, 78 per cent. The professional staff loses impact ground in the second run and the cottage staff gains. Such findings are in accord with the other studies, although it would be expected that in view of the closeness of the professional worker to the child in this residential diagnostic centre, the impact of such workers would be a lot higher in this situation than at the penitentiary, the reformatory, or the training school for delinquents. Zibners concluded that the cottage workers exercise greater influence on the children than the professional workers (the psychologists), even when the latter have a close working relationship with the subjects.

Moran's Study. Mark R. Moran used a standard terminal interview with 250 consecutive releasees from the U.S. Reformatory at Chillicothe, Ohio, in the summer 1953, in order to gauge the inmate's feelings and concepts about himself as an inmate in a progressive reformatory, to sense his feelings and concepts about inmate society of the reformatory, and to sound out his feelings and concepts about the staff of the reformatory.⁶ Here we have an impact study with a different pitch. One might say that if the inmate person overwhelmingly conceived of himself as a convict, looked upon staff as sword rattlers, and felt that inmate society exerted strong-arm pressures of an exploitive variety, the climate would be more like an old-time prison, certainly not conducive to constructive impact. Only some of Moran's specific findings will be cited.

'Is a fellow able to keep his self-respect in this institution?' Answers: all of the time, 71 per cent. (of the 250 inmates); some of the time, 26 per cent.; not much of the time, 3 per cent. 'Do the inmates here think they are better off than the inmates in a state prison?' A lot, 48 per cent.; quite a bit, 21; some better, 30; no answer, less than 1 per cent. 'Do inmates here think they are lower than a G.I.?' (Remember that 60 per cent. of them had had armed service experience.) Yes, 50 per cent.; no, 49; no answer, less than 1 per cent. 'After you got used to the place here, which one of the following best describes how you thought of yourself during your stay here?' Student, 58 instances; trainee, 64;

⁶ Mark R. Moran: 'Inmate Concept of Self', Ph.D. Dissertation, The Ohio State University, 1954.

patient, 9; about like I always felt, 35; pulling time, 81; criminal, 3. The first three are considered favourable concepts and give a cumulative total of 52 per cent. of the 250 answers. 'Pulling time', a sort of convict concept, comprised one-third of the answers. 'Did you think of yourself as a convict during your stay here?' Not much of the time, 70 per cent.; some of the time, 23; all of the time, 7. 'Do the personnel here try to make the fellows think they are convicts?' None do, 43 per cent; one or two do, 38; more than two do, 19. 'Do the personnel here throw their weight around?' None do, 30 per cent.; one or two do, 53; more than two do, 17. 'Are there "sweet boys" and their "daddies" around here?' Very few, 60 per cent.; some, 32; a lot, 8. 'Does strong-arming happen here among the inmates?' Not at all, 44 per cent.; very little, 35; some, 21. 'Is the inmate council here any good for helping a fellow get adjusted?' A lot of good, 57 per cent.; some good, 34; not much good, 9.

We can see from these answers that the inmate's concept of himself, the staff and inmate society at this particular reformatory is good—certainly conducive to social rehabilitation. Such answers seem to the writer to be consistent with the original sort of impact soundings which were made during the same terminal interviews at the reformatory and already reported under 'Chillicothe Revisited'.

Conclusions. Answers to questions such as what inmate persons have got out of their institutional stay, what staff member has helped them the most, what part of the programme benefited them most, can form the basis of gauging the impact of penal and correctional institutions on inmates. If staff members are close enough to inmate persons and have common denominators of thinking relative to improvement shown by inmates, their judgments on what the inmate has received from his institution experience can also be used as a measure of impact. Soundings do not have to be limited to the total benefit of the programme. They can be focused on any particular part of the programme—say, for example, the inmate council, classification procedures, preparation for release, etc. The overall operational effectiveness of the total programme or the specific effectiveness of a specific programme within the total can be sensed by such methods. These impact soundings can be a guide to administrators. Certain staff members can be better wheeled into action. Certain measures can be redesigned.

At this juncture we are certainly not in a position to generalize. Impact studies, if they take hold and find their way into widespread use, can best function to gauge what is happening at a certain institution or in a specific programme within a certain institution. However, as experience cumulates, it might be possible to make safe generalizations about the effectiveness of certain kinds of programmes or staff on certain kinds of inmates. At least, if we project ourselves far into the future, we can contemplate the possibility that through impact studies penology can become a science—a sort of applied behaviour science, engaged in the study of how individuals respond to authoritarian settings. The only other trend which has given promise of lifting penology out of its doldrums is the prediction movement, which many administrators and social workers thoroughly dislike but which could lead to a scientific basis for screening and selection of offenders for certain measures of treatment. It would certainly be worth the effort to move penology out of its emphasis on penal reform and into a behaviour science which can gauge impact of penal and correctional programmes on offenders.

Previous and Contemporary Efforts. In their first follow-up study the Gluecks

tried to disentangle the 'influence of the reformatory', by comparing a group of 128 subjects 'who committed serious offences prior to their sentence to the Reformatory and continued to commit such offences during the parole and post-parole periods,' thereby constituting the uninfluenced group, with a group of 129 subjects 'who committed serious offences prior to their sentence to the Reformatory, whose conduct showed some improvement during parole (namely, the commission of minor offences only or a change in classification from total failure to partial failure or success), and also showed improvement during the post-parole period,' thereby constituting the influenced group.⁷ But on the basis of this comparison a determination was made of what pre-reformatory (background) factors facilitate and what factors hamper the impact of the reformatory. The Gluecks found that being a skilled rather than an unskilled worker, being a partial success at meeting the economic obligations of the family rather than a failure, and having no prior penal experience rather than having prior penal experience, were the principal factors which facilitated favourable impact of the reformatory. The Gluecks also made a similar analysis in their research on 'Five Hundred Delinquent Women', which was their second follow-up study.⁸ Returning to the first study, the Gluecks drew close to the present effort to gauge impact when they attempted to make soundings of the beneficial effects of reformatory life from expressed opinion of 301 of the ex-reformatory inmates five years after release. Sixty-eight per cent. (205) of them made 294 favourable statements. Thirty-seven per cent. of these 294 favourable comments were made by men who were total failures; 22 per cent., partial failures; and 38 per cent., successes in the post-parole period. Ninety-six of the 301 ex-inmates (32 per cent.) made 136 unfavourable comments about the reformatory. Of these 136 statements, 77 per cent. were made by men who were total failures; 12 per cent., partial failures; and 11 per cent., successes in the post-parole period.⁹

Raymond Corsini conducted a standard interview with 50 consecutive inmates who had received their dates for parole release in Auburn Prison (N.Y.) in 1942-43. Most of the 12 standard questions were impact questions. The answers to three of them are of particular interest at this point. 'Have you been reasonably happy in Auburn prison?' Twenty-five inmates answered yes, and 25 no. 'Do you think that you have benefited by your stay at Auburn?' Thirty said yes: 7 no; 3 yes and no; 10 refused or gave ambiguous answers. 'Did any person help you to understand yourself while in prison?' Sixty-four per cent. professed that they had been helped; 36 per cent. that they had not.¹⁰ This is pretty fair impact for a maximum-security prison which was not designed or staffed for social treatment.

Special training units were established in 1941 by the British Army for soldiers under 21 who were rather persistent run-away problems and discipline cases in the regular outfits. The operational theory was that such offenders 'could gradually be led into sociable ways' by understanding the individual man, relaxing the

⁷ Sheldon and Eleanor T. Glueck: 'Five Hundred Criminal Careers', New York, 1930, pp. 225-6.

⁸ Sheldon and Eleanor J. Glueck: 'Five Hundred Delinquent Women', New York, 1934, pp. 264-83.

⁹ 'Five Hundred Criminal Careers', *op. cit.*, pp. 298-301.

¹⁰ Raymond Corsini: 'A Study of Certain Attitudes of Prison Inmates', *Journal of Criminal Law and Criminology*, 1946-7, 37, pp. 132-5.

pressure of ordinary discipline, appealing to a man's 'better nature', developing his education and physique, and giving attention to 'important details of hygiene and social habit'.¹¹ The original design of each unit was for a group of 240 trainees with a staff of 60. Joseph Trenaman observed and studied the results of this re-training at one of the units in 1944. Several impact questions were asked the trainees at one point in the inquiry. Forty per cent. admitted that they intended to go AWOL on arrival. Actually only 4 per cent. did. 'Why do you think the Army has set up Special Training Units?' Three-quarters of the trainees gave reasonably correct answers. 'Do you think the staff is trying to be fair to you?' Ninety-three per cent. replied yes. 'Is the unit better or worse than you expected it to be?' Eighty-one per cent. answered better. 'What part of the training here do you like best?' Answers: education, 35 per cent.; physical training, 30; drill and assault course work, 35. 'Is it easier to keep out of trouble here than elsewhere?' Sixty-three per cent. answered yes. Actually, the ratings and conduct reports on the trainees when returned to regular Army units were very satisfactory.

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NOTE: The subject of the studies described by Professor Reckless is of great importance, but has so far been given but little attention in this country. One of the few exceptions is Dr. Alexander Leitch's paper, 'A Survey of Reformative Influences in Borstal Training' (*The British Journal of Medical Psychology*, Vol. XX, Pt. I, 1944, pp. 77-95). Reference might also be made to an American study briefly reported in this Journal: Raymond J. Corsini and Kenwood Bartleme, 'Penology and the Attitude of Prisoners', *Brit. J. Delinq.*, Vol. IV, No. I, July 1953, pp. 55 *et seq.* (Ed.)

INTELLIGENCE, CRIMINALITY AND THE EXPECTATION OF RECIDIVISM¹²

The last half-century or so has witnessed a growing interest in the psychological approach to the problems of causation in crime and delinquency. It is natural, then, that investigators should for some time have concerned themselves with assessing the intelligence of criminal and delinquent groups, the cognitive aspects of personality being, generally speaking, the easiest to assess. It is strange, however, that there is still disagreement on this point. The suggestion has been made that the reported frequency of cognitive defect among criminals has dropped as intelligence testing has become more reliable and standardized, and certainly early investigators seem to have suffered badly from halo effects in their judgment of what constituted intellectual subnormality, chiefly, one suspects, through a failure to separate the concepts of mental and moral defect. Clearly, it was not very difficult to establish a high degree of moral defect among criminals, and since this state was not clearly differentiated from intellectual defect as we understand the term to-day, it was not surprising that criminals were judged to be of inferior

¹¹ Joseph Trenaman: 'Out of Step', New York, 1952, p. 3.

¹² The author is grateful to the Prison Commissioners for permission to publish this article, which does not, however, necessarily represent official views.

intelligence. But this cannot be the whole story, since equally competent investigators have continued to disagree even since the advent of reasonably adequate tests. However, it is not intended to speculate here on the reason for discrepancies in the previous literature, but to offer some evidence based on a study of test scores of prisoners who are serving or have served sentences at Wakefield Prison. This evidence must be considered suggestive rather than conclusive—I cannot claim conclusiveness because it was not possible to compare the prisoners with a normal non-criminal group, so that we were confined to making comparisons within the criminal group. However, if we set up the hypothesis that criminals are significantly less intelligent than the normal population, it seems a reasonable deduction that, within a criminal population, the worse the criminal, *i.e.* the blacker his record, the lower his intelligence. This was the kind of deduction which the present work set out to test.

We shall first consider data obtained from the test scores of receptions into the prison during the two-year period 1952–53, many of whom will, of course, still be serving their sentence. Let us first consider the overall distribution of intelligence test scores for this population: as already indicated, no normal control group was available, but it was possible to compare the actual test results with ‘expected’ frequencies based on the test norms. Raven’s Matrices Test¹³ was used, for which norms have been established, on the basis of five grades, from very bright (A) to very dull (E).

TABLE I
(percentages in brackets)

	<i>Actual Frequencies</i>	<i>Expected Frequencies</i>
A . . .	90 (8.3)	107.9 (10)
B . . .	246 (22.8)	215.8 (20)
C . . .	394 (36.5)	431.6 (40)
D . . .	208 (19.3)	215.8 (20)
E . . .	141 (13.1)	107.9 (10)

$\chi^2 = 10.92$ (if allowance is made for the ‘progression of signs’ the probability of χ^2 might be slightly smaller.) $P < .05$.

There is thus some tendency for the scores to pile up at the low end, and the Chi-square value is significant. So far as can be seen, then, without a control group, this population appears to be rather less intelligent than the general population; although the fact that the lower social strata are probably disproportionately represented in the criminal population should not be overlooked.

It is instructive, however, to examine the distribution of scores *within* the group. To this end, the population has been divided into three categories—sex offenders, which include hetero and homosexuality, violence offenders, which include assault, murder and manslaughter (but exclude sexual assault and robbery with violence), and thieves, which include housebreakers, common thieves and robbers. False

¹³ Receptions into this prison are given a further intelligence test, the results from which are substantially similar to those obtained from the Matrices.

pretence offenders have been left out, as have other small groups, but the overall picture is not thereby affected. This threefold classification takes up nearly the whole of the population. Test scores have likewise been classified into three categories—average, above average, and below average. It has therefore been possible to construct a 3×3 contingency table, to show relationship between intelligence and type of offence.

TABLE II
(percentages in brackets)

	<i>Sex</i>	<i>Violence</i>	<i>Theft</i>	
Above . . .	(29·3) 101	(23·1) 33	(33·5) 157	291
Average . . .	(31·2) 107	(35·7) 51	(39·8) 186	344
Below . . .	(39·5) 136	(41·2) 59	(26·7) 125	320
	344	143	468	955

This table gives a contingency coefficient¹⁴ of about ·15, which is small, but statistically significant. The implication is that the thieves are slightly more intelligent than the other groups. Yet they are very definitely the most criminally inclined; the best objective evidence of this is the number of previous convictions. The following 3×3 table shows the relationship between type of offence and number of previous convictions:

TABLE III
(percentages in brackets)

<i>Number of previous convictions</i>	<i>Sex</i>	<i>Violence</i>	<i>Theft</i>	
0 . . .	(64·2) 223	(41·6) 57	(22·4) 104	384
1-2 . . .	(24·8) 86	(37·2) 51	(26·7) 124	261
+2 . . .	(11·0) 38	(21·2) 29	(50·9) 236	303
	347	137	464	948

¹⁴ A measurement of association suitable for data of this kind.

(The slight difference in totals is due to the fact that for a few prisoners complete data were not available.) This table shows the much more substantial contingency coefficient of .42, and proves beyond doubt the worse criminal records of the thieves. Yet, as we have seen, they are definitely not less intelligent than the other groups, and in fact may be slightly more so (their slight superiority in intelligence is probably explicable by the comparative youth of this class of offenders: intelligence, as is well known, shows a decline with age).

Consistent with the above findings are the insignificantly low correlations we have found between intelligence and: (a) Frequency of criminal conviction; (b) Age at first recorded conviction; (c) Approved School and Borstal failure.

To sum up: there is no connection between intelligence and 'badness' of criminal records. To put the same point another way, since the factors listed above have been found to be associated with recidivism, we may conclude that there is no reason to believe that our most likely recidivists will be drawn from the ranks of the least intelligent.

Lastly, we shall look at a past population, which has already been discharged from the prison. This is a group of nearly 800 prisoners¹⁵ which has been written up by Dr. W. F. Roper¹⁶; all of the men of this group have been released at least three years ago. As with the present population, it has been discovered that there are, for the most part, no significant correlations between intelligence and the various indices of the 'badness' of criminal records; and, as with non-criminal groups, intelligence has been found to vary positively with social level, and negatively with the size of family of origin. But our most interesting finding so far as this group is concerned is a lack of correlation between intelligence and recidivism—i.e. the 10 per cent. or so of this population which has been reconvicted since discharge from Wakefield are no less intelligent than those who, at any rate as yet, have kept out of further trouble. Clearly, then, so far as the Wakefield population is concerned, intelligence level offers no basis for the prediction of recidivism.

In the absence of a control group, we are not entitled to say anything definite about the intelligence of criminals as compared with normals. But we have shown fairly conclusively that, *within* a criminal group, there is no tendency for the most criminal to be the least intelligent. This does not, of course, disprove the hypothesis that criminals as a whole are less intelligent than non-criminals. Nevertheless, if that hypothesis were true, it would, I think, have been reasonable to expect that the most anti-social of our prisoners *would* have been the lowest in intelligence—but, as we have seen, this is not so.

It must be admitted that the Wakefield population has been highly selected, and cannot be considered typical of the prison population as a whole; Wakefield prisoners are, broadly speaking, selected on the basis of their trainability, a factor which may well be positively correlated with intelligence, and which would therefore send up the intelligence level in comparison with a more typical population. But this cannot alter the fact that the potential—and, in the case of those reconvicted, the actual—Wakefield failures, who may be said to approximate most closely to the norm of the habitual criminal population, are no less intelligent than the most hopeful of our prisoners.

¹⁵ The data quoted on this population are part of a larger research project, not yet completed.

¹⁶ Roper, W. F. 'A Comparative Survey of the Wakefield Prison Population in 1948.' (This Journal, Vol. I, No. 1, July, 1950.)

Finally, we may be permitted a word about the wider implications of the results. We have been examining a group of men whom society has labelled 'criminals'. Now crime is a legal and not a psychological concept; nevertheless, it is probable that criminal behaviour does correlate with some psychological trait—e.g. emotional maturity, frustration tolerance, etc. If whatever we like to call the psychological attribute associated with anti-social behaviour is a quality independent of intelligence, then clearly we are faced with a fact of both theoretical and practical importance.

B. MARCUS.

NOTES AND CRITICISMS

THE INSTITUTE FOR CRIMINOLOGY AT THE UNIVERSITY OF OSLO

In 1954 an institute for criminology and criminal law under the direction of Professor Andenaes was founded at the University of Oslo in the faculty of jurisprudence.

Active research is under way in Norway at present in this field, especially on the sociological and juridico-sociological side, the public authorities are very co-operative, and the National Research Council has granted funds to a number of young research workers—jurists, sociologists, psychologists—enabling them to undertake full-time research projects. In certain respects criminological research is in a better position in a small country than in a larger one: it is easier to obtain a complete picture of criminality, and there are better possibilities of eliminating emergent sources of error. Several of the research projects are intended to show how the system of societal reaction to crime actually works. One investigator is working on a juridico-sociological dissertation concerning the use of detention before trial. Another has taken up problems connected with the imposing of fines and the employment of subsidiary prison sentences for unpaid fines. A third is studying the activities of the department of child welfare, with a view to clarifying the relationship between the ideology on which the law is based and the practices actually followed. Other research workers are engaged in projects of a purely crimino-sociological and crimino-psychological character.

A practical and important issue arises regarding the position of criminology in juridical education. Criminology is not an obligatory subject in the Norwegian Law School, but instruction in criminal law, which is considered one of the central disciplines in legal education, gives considerable scope to an introduction of the elements of criminology and penology. In addition to the obligatory subjects, each student chooses a special line of study from among several optional fields. Criminology (together with legal psychiatry and penology) belongs to these optional subjects, on the same basis as, for example, the disciplines of tax law, maritime and insurance law. It is one of the most popular choices. Graduates within this special field often obtain posts in the department of prisons, with the public prosecuting authorities, or in other branches of the administration of criminal law. In this way it is hoped in time to have connected with the administration of criminal law jurists who bring from their studies a fairly thorough schooling in criminological and penal problems.

JOHANNES ANDENAEAS.

CRITICAL NOTICES

STRAFUDMALING (*The Assessment of Punishment*).¹ By W. E. v. Eyben. (Copenhagen: G. E. C. Gads forlag. Pp. viii + 502.)

Modern criminal laws as a rule afford the judge considerable freedom to assess the punishment for a given crime, or to replace the usual penalty by other measures. Theoretically, at any rate, the fact that the accused is found guilty may be said to have less influence on his fate than the point on the scale of punishments where the judge's finger stops. Criminal cases seldom give rise to difficult juridical problems; but the question of the due assessment of punishment arises in every single case. This being so, it is surprising how little the principles of judicial sentencing have been discussed in the literature of criminal law. Undoubtedly this is due to the fact that such problems are not well suited to treatment by traditional juridical methods. Statutory prescriptions for what the judge shall take into consideration when assessing punishment are seldom found and when found are seldom instructive. Moreover courts rarely explain in detail the reasons for their choice of penalty.

W. E. von Eyben's main task is to show how the present system of assessment of punishment actually functions. Of what significance, for instance, is the range of penalties prescribed by law, and how does a change of maximum and minimum provisions affect the meting out of punishment? What are the long-term tendencies of change? In what respect does apportionment of punishment vary from one country to another? What factors actually determine the length of a sentence? The work under consideration is as much a study in the sociology of law as in jurisprudence. To illustrate the assessment of punishment in action the author has not been content to use only published legal records, which one cannot assume are representative of the great mass of cases; with the object of including *all* decisions arrived at within a certain period, he has made use of 3,690 unpublished sentences pronounced by Danish courts. He has also sent out questionnaires to criminal court judges in several countries, and undertaken a comprehensive analysis of criminal statistics of sentences, especially in Denmark and Germany. The author—who has since been appointed professor of civil law at the University of Copenhagen—has also drawn upon his own experiences as a judge in the city court of Copenhagen.

As far as the reviewer knows, this book is the most comprehensive account of the assessment of punishment to be found in any language. Not all of the author's conclusions are equally convincing, but the technique of his approach and the wealth of material adduced make it a significant contribution. In Scandinavian countries it has given rise to lively controversy. The book includes a short summary of its argument in English, but this gives only a poor impression of its content. The author's investigations are limited in the main to the Scandinavian

¹ We regret the delay in publishing a review of this important book, which reached us in 1953. It was only after some unsuccessful attempts to review it on the basis of the English summary it contains that the present reviewer was approached. We are very grateful to Professor Andenaes for producing his Critical Notice so promptly. (ED.)

and Continental countries. In the case of England he confines himself to a discussion of Radzinowicz's article on 'The Assessment of Punishments by English Courts' in *The Modern Approach to Criminal Law*, 1945.

It has been considered almost axiomatic that punishment tends to become steadily more lenient. This is clearly true if comparison is made with the situation a hundred years ago or more. The consensus of opinion is, however, that this lenient tendency has persisted up to the present day. The prominent German jurist and criminologist, Franz Exner, published apparently conclusive evidence based on a study of German criminal statistics from 1886 to 1928. In his book 'Studien über die Strafzumessungspraxis der deutschen Gerichte' (1931), Exner concluded that the tendency is towards moderation all along the line; from imprisonment to fines, and from strict and lengthy sentences of imprisonments to more lenient and shorter ones. His explanation of the phenomenon is also quite convincing. It is partly due, he thinks, to growing scepticism regarding the reforming effect of deprivation of liberty, partly to changing attitudes toward crime and criminals. It has been increasingly recognized that crime is a result of inborn traits and of environmental influences over which the individual has been able to exercise no choice. Such realization cuts the ground from under moral indignation over the crime. Consciously or unconsciously, this changed attitude influences judges, prison authorities, and legislators in the direction of leniency. Exner also expressed his conviction that the movement was worldwide; and he pointed to similar tendencies in Austria, France, and England. Von Eyben gives an account of later research, including Radzinowicz's investigation of England for the years 1900-36, which seems in general to support Exner's conclusions.

Von Eyben is, however, very sceptical of the evidential value of these studies. He points out the obvious error that arises when all crimes are treated together, as by Exner, also that the standards of crime may have changed during the period under investigation. The types of crime dealt with in German criminal statistics increased in number from 550 before 1914 to 790 in 1927. As von Eyben indicates, when conduct formerly unpenalized is made subject to punishment, the courts avoid applying deprivation of liberty as the normal penalty. Even if the material is classified according to types of crime, one cannot be certain that the composition of the material has not changed. The definition of a crime may be altered, or even though the definitions remain the same, changed conditions in society may give rise to new types of transgression. Changes in procedure and variations in the practice of the prosecuting authorities may also influence the selection of such punishable acts as appear in court.

On the basis of these methodological considerations, von Eyben concludes that the evidence does not fully support the existence of a general tendency towards greater leniency, provided the material remains unchanged; but he records a steadily growing use of the suspended sentence and a certain falling off of very long terms of imprisonment, which he maintains are connected with a changed conception of the problem of the recidivist. Von Eyben's methodological criticism of previous investigations may well be justified. Absolutely conclusive evidence can be worked out only by means of more refined analyses. However, his critique seems somewhat out of proportion. In my opinion it can hardly be doubted that his views are in the main correct, in regard to both the actual development and its causes.

One of the objections which has been raised to the traditional assessment of punishment is that it is *inconsistent*, *i.e.*, that it is dependent on the individual judge. The author investigated the significance of the personalities of individual judges by means of a questionnaire, describing imaginary criminal cases, which was sent to a number of judges in Denmark, Norway, Sweden, the Netherlands, and France. The answers demonstrate considerable variations even within the same country. For the same offence the penalties suggested vary, for example, from 80 days' suspended sentence to 18 months' imprisonment. For this reason von Eyben maintains that one ought not to leave sentencing so largely to the discretion of the courts, unless the judges are more specifically instructed in the assessment of punishment than is now the case.

I am not convinced that such broad conclusions can be drawn from his material. Von Eyben himself points out certain sources of error, among others that the description of cases in the questionnaire was so brief that the judges who replied had to use their imagination to fill out the picture. The kinds of behaviour and personality-types which they had in mind when answering can thus have varied considerably. In general, it may be assumed that those judicial sentences which vary most from the norm are more often than not appealed against by one party or the other. The fact is that such great variations as the questionnaire answers show are seldom to be found in published court reports, at least in Norway. Neither does the author draw the practical conclusion that the legislature ought to restrict judicial freedom of choice; on the contrary, he maintains that the bench should be as unhampered as possible and should take into consideration the special circumstances which arise in each individual case.

Von Eyben is, however, completely right that expression of the judge's personal reactions should be avoided as much as possible, and that the remedy lies in better knowledge of judicial practice. In Supplements A and B the author has systematized his material, arranging the sentences according to the type of offence. I believe such a systematization of judicial practice can be very useful, provided it is not considered a mere schedule to be consulted mechanically in order to find the penalty for a given offence. A well-organized compilation of this kind could at any rate prevent those expressions of the judge's individual bias which arise from lack of knowledge of usual judicial practice.

Analysis of his data on court sentences leads the author to the following categorical conclusion: punishment is the product of two main factors, the seriousness of the offence and the nature of previous convictions, *i.e.*, number, duration, and difference between previous offences and the crime under immediate consideration. The most important point for the judge is neither the legal range of penalties and regulations for their assessment, nor rational weighing of the general and particular preventive considerations at stake, but *custom*, the *tradition of assessment of punishment*. In the words of the Swiss author Pfenninger, the assessment is not a rational but a traditional reaction. Von Eyben adds that irrational and purely emotional factors can also take part—a point which in my opinion deserves more thorough discussion.

The author does not explain what he means by 'the seriousness of the offence', but his train of thought seems to indicate that he is thinking of *objective seriousness*. There can, however, be no doubt that the meaning of the term 'seriousness of the offence' also leaves room for subjective circumstances of an aggravating

and extenuating kind, for example, age and mental condition. The author's view is perhaps best summarized in the following quotation: 'The assessment of punishment gives concise expression to the fact that the punishment expresses societal disapproval, and that the severity of the penalty indicates the strength of the disapproval.' This formula also includes consideration of any earlier convictions: the criminal has not been influenced by the previous expression of disapproval on the part of society, and must therefore expect that the next sentence will be more severe.

In the author's opinion one must expect this tendency to persist. 'This investigation has resulted in deep pessimism regarding any change in assessment of punishment according to rational, criminological viewpoints.' The course of future development must be that more and more cases are released from 'the area of punishment', so that special measures, whose duration is not related to guilt, take the place of penalization. The author himself admits that his account is conservative and that it is useless to make proposals which cannot be expected to be followed. His recommendation is in a way negative: he suggests that minimal and maximal penalties be abrogated, because he considers them at best superfluous, at worst—namely, if they inhibit the judge's power of assessment—directly harmful. The assessment of penalties is and must be a judicial function; legislative tables in the form of minimum and maximum penalties are worthless; the decisive factor is that schedule of penalties which has grown up in practice.

This analysis of the assessment of punishment in my opinion presents important factors which usually are not evaluated realistically enough. Everyone who has had practical experience in this field knows that the task of meting out punishment usually consists not in independent appraisal of what might best deter future offences either by the individual involved or by the general public, but in working out a suitable punishment according to the customary assessment norms. I feel, however, that von Eyben overemphasizes the opposition between the rational and the traditional. I believe it is comparatively seldom that a judge feels such incompatibility. The reason is simply that it lies beyond the judge's power to determine a given penalty on the basis of rational deliberation; he is therefore driven to seek support in tradition. We know very little about the general deterrent effect of a heavier or lighter punishment; especially we know little about the significance of different principles of gradation within the legal range of punishment. Neither do we understand much about reformation of the individual offender. We know little of what positive or negative significance punishment has for the criminal; still less ground do we have for judging the probable influence of a penalty of 30 days, for example, instead of 3 or 6 months.

Under these conditions it is completely logical for a judge to reason thus: it would be all too fortuitous and uncertain if I should make my decision on the basis of an independent weighing of the different purposes of punishment in each separate case; it is better that I keep to current practice.

The 'system of rates' becomes irrational when the judge feels himself bound by the usual assessment of punishment in similar cases, or if he makes his decisions automatically without considering whether or not this leads to wise handling of the actual situation. Where reasonable treatment of a human being demands a solution other than the customary penalty, it should as a rule be the latter which yields.

JOHANNES ANDENAES.

THE TRIAL OF CHRISTOPHER CRAIG AND DEREK WILLIAM BENTLEY. Edited by H. Montgomery Hyde, M.P., Barrister-at-Law. (London: William Hodge & Co.Ltd., in the 'Notable British Trials' Series. 1954. Pp. 264 (with Appendices). Price 15s.)

On November 2nd, 1952, two young men, Christopher Craig, aged 16½, and Derek William Bentley, aged 19 years, were found on the flat roof of a warehouse in Croydon which they were about to break and enter. The former was armed with a knife and a fully-loaded revolver, the other carried a knife and a knuckleduster. Several police climbed to the roof and endeavoured to arrest them. Bentley was seized by one of them but he broke away, and it was said that he shouted to Craig, 'Let him have it, Chris.' Craig fired and wounded the policeman, but with great courage this officer again seized Bentley and held on to him despite further shots from Craig's revolver. Finally, another policeman who came up through the inside of the warehouse and out through a door leading to the roof was shot dead by Craig, who then jumped off the roof and fell to the ground, where he was arrested. Both Craig and Bentley were charged with murder, and were tried at the Central Criminal Court (the Old Bailey) and convicted, Craig being sentenced to be detained during Her Majesty's pleasure, because he was under the age at which the capital penalty is enforced, and Bentley being condemned to death. Craig did not appeal, but Bentley's appeal to the Court of Criminal Appeal on the ground of misdirection failed, and the Home Secretary, in the face of considerable public pressure and despite the jury's recommendation to mercy, did not recommend a reprieve, so that Bentley was hanged.

This represents a brief outline of the events in a case which, as the authors of the book of the trial say, 'may well prove to be the high-water mark of the tide of youthful criminal violence in the post-war years' (p. 3). The national conscience was at last awakened to the dreadful realization that there had arisen in our midst scores of young men who, with the gangster film and the horror comic as their inspiration, revelled in acts of violent crime and anti-social behaviour. One immediate result was the passing of a statute, the Prevention of Crime Act, 1953, which makes it an offence to carry an offensive weapon without lawful authority or excuse, and gives the police power to stop and search suspected persons. This was designed to prevent persons carrying such horrible weapons as the dagger-type knife and the spiked knuckleduster which were found in Bentley's possession.

But the case has significance not only as a reflection of the activities of lawless youths and a goad to the public conscience, but also from the legal angle, as a rather unfortunate example of the strictness of the English law relating to homicide. It is perhaps for this that it will be most remembered. There is not much question about Craig's having been properly convicted of murder, but Bentley's conviction and subsequent execution are noteworthy indeed, if not notorious. It was widely thought to reveal the astonishing harshness of some parts of our criminal law, which, it was said, have never quite caught up with modern ideas, and the decision not to grant a reprieve produced a wave of protest not only in Parliament and in the press, but in other parts of the civilized world where such a result would never have been reached in similar circumstances.

It is therefore good to have on record in this volume the proceedings of the trial

at the Old Bailey, at which Lord Goddard, the Lord Chief Justice, presided, and the appeal in the case of Bentley (Appendix I), and also the proceedings in the House of Commons on the eve of the execution (Appendix II), together with a masterly introduction by so experienced a trial book editor as Mr. H. Montgomery Hyde, M.P. A careful study of this volume should serve to dispel some at least of the most extravagant criticisms of the state of English law or the conduct and result of the trial. What remains is an uneasy feeling that justice in her blindness was not really even-handed in relation to these two youths, and that the prerogative of mercy might well have been exercised to save Bentley. But at the same time the reader of this volume will be inspired with profound admiration for the conduct of the police and will possibly concede that it is not surprising nor unreasonable in these special circumstances, when a policeman has been needlessly killed, that the Home Secretary should be disinclined to interfere with the course of the law. Perhaps the case demonstrates once again the futility of trying to do justice under the shadow of the capital penalty.

A few words may be permitted on the merits of the argument in defence of Bentley. It was contended that he did not know that Craig was armed and intended to use the weapon, and that even if he did know this, and had freely joined in a criminal enterprise with Craig, their joint adventure had terminated when Bentley was arrested on the roof. Unfortunately for this argument, however, Bentley strongly denied in evidence that he was under arrest at the time when Craig fired the fatal bullet, and it is almost certain that he was not under arrest when he incited Craig to fire the first shot by shouting 'Let him have it, Chris.' Of course, he disputed having said this, but the evidence of the three policemen who heard it was unshaken. Moreover, he had not taken any step to dissociate himself from the joint adventure during the time he was standing by the policeman on the roof-top. He had retired from the fray, but he made no effort to influence Craig to give himself up or stop shooting. In these circumstances, the application of the rule of law was pretty clear. Lord Goddard was surely expressing the law correctly when he said: 'Where two people are engaged in a felonious enterprise . . . and one knows that the other is carrying a weapon, and there is agreement to use such violence as may be necessary to avoid arrest, and this leads to the killing of a person, or results in the killing of a person, both are guilty of murder, and it is no answer for one to say "I did not think my companion would go as far as he did."' The Court of Criminal Appeal, presided over on this occasion by Mr. Justice Croom-Johnson, were quite emphatic in rejecting the notion that the defence had not been fairly put to the jury by Lord Goddard, and on the question of Bentley's share in the responsibility, they simply said that it was not open to Bentley to claim that the criminal enterprise was at an end when he himself had asserted that he was perfectly free and not under arrest at the time of the killing.

As has been said, this should dispose of the wilder criticisms of the legal aspects of the case. It is no use speculating what the position might have been had Bentley not shouted those words of incitement to Craig, or had he been definitely under arrest, or had he emphatically dissociated himself from Craig's conduct. That is not a matter which can be usefully discussed. But taking the case on its accepted facts, the thought remains that the law dealt somewhat harshly with Bentley, particularly in view of Craig's legal position, which entitled him to escape the capital penalty. It is understandable that persons unfamiliar with our legal

system should have been profoundly shocked by the result of this case, and that even those who know it well should still feel a trifle uneasy about it.

The criminological interest of the case lies in the evidence that both these youths were below average intelligence, and, despite the fact that they came from good families, they had both been difficult children. Craig never learned to read or write properly and, apart from his addiction to gangster films, showed an abnormal interest in firearms, which he used to carry because he said they made him feel important. Shortly before the night of the fatal shooting, his brother had been convicted at the Old Bailey on a charge of armed robbery, and sentenced to twelve years' imprisonment. Bentley had previous convictions for shop and store breaking, and had been to an approved school at the age of 14. He was unemployed at the time of the crime. It was he who provided the physical strength and Craig the brains in this unhappy partnership. He was extremely proud of his powerful physique and liked to pose for 'strong-man' photographs in the back garden of his home. Perhaps the most significant fact which the psychiatrist would see in Bentley's history is that he had had a bump on the head when he was a child. He had been bombed out twice during the blitz, and on one of these occasions he had been dug out with head injuries. What a pity that we deprive our psychiatrists of some of their most interesting material by exacting the capital penalty!

J. E. HALL WILLIAMS.

REVIEWS

PROCEEDINGS OF THE SECOND INTERNATIONAL CONGRESS ON CRIMINOLOGY. Vol. II : Biology and Anthropology. (Paris: Presses Universitaires de France. 1952. Pp. 408.)

The attempt to attribute criminal conduct to constitutional factors had its first scientific and somewhat dramatic expression in Lombroso's work. To-day we ask for more statistical checks than Lombroso employed and for a wider and more scrupulous selection of the multiple factors.

Kretschmer, whose paper opens this volume, gave us the first modern approach to somatotyping in relation to character, but his sketchy methods of visual scrutiny have been further dimmed by more scientific sampling and statistical analysis. Dr. Snagrosse carefully examines both Sheldon and Hooston, who have attempted to reinstate the concept of constitution as against the environmental forces making for criminal behaviour. No one can deny the *a priori* arguments for a close correlation of body-mind linkages, but Hooston's approach betrays statistical shortcomings, *i.e.*, errors of sampling and the exclusion of, for example, the nutritional factor, which play tricks with somatotype formulations.

In this connection the French papers by Pierre Grapin and Eugene Schreider merit close attention. Here careful physical anthropological indices are used and the insidious attribution of racial factors is examined (Verdeaux). Indications of the complexity of the subject are displayed in the search for neurological factors, *i.e.*, diencephalic functions, electrophysiological (Journay; Verdeaux and Lairy-Bonniers).

It is difficult to cover all the articles derived from many countries, but it can be said that they all display interest and skill in the handling of factors. There is, however, considerable overlapping and, in fact, reduplication.

E. M.

PROCEEDINGS OF THE SECOND INTERNATIONAL CONGRESS ON CRIMINOLOGY. Vol. III: Psychiatry, Psychology, Psycho-analysis. (Paris: Presses Universitaires de France. 1952. Pp. 408. 1,900 fr.)

By the time this somewhat belated review appears we shall be in the throes of the Third International Congress, a fact which might appear to detract from the value of any critical notice of the present volume. As, however, the Third Congress will presumably produce its quota of printed Proceedings it may not be altogether out of place to appraise the value of the current production, which consists of three main parts, viz., international reports, discussions of the same, and a number of individual communications.

It may be said at once that these are of very unequal merit. As far as bibliographical surveys are concerned only the contributions from the United States are worthy of the name. Drs. Bromberg, Karpman and McCorkle together with Messrs. Rourke and Shulman are indeed to be congratulated on giving a clear and informative account of the progress of forensic psychiatry, psychology and psycho-analysis during the years 1938-50. This section alone gives the volume some claim to distinction as a book of reference. To be sure, there is a valuable and reflective general commentary on the proceedings of the whole section by Dr. Carroll. This deals with causes and methods, but otherwise only brief reference is made to work in Britain. France is represented by two reviews heavily weighted by psycho-analytical thinking. The contributions from Argentine, Belgium, Italy and Switzerland cannot be called surveys. The rest of the international field is unrepresented!

Regarding the second part of the Proceedings, which consists of reports of sectional discussion meetings, the less said the better. Congress discussions are notoriously windy, even more so than discussions following papers delivered at private societies. And this particular congress was no exception. There seems to be no justification for expending the Association's exiguous finances in such fruitless effort. Reports of this kind should be cut to the bone by a flint-hearted editor, who could easily condense to a few pages the more fruitful suggestions made in course of discussion.

The twenty communications with which the volume closes cover a wide field; like the general reports they are of unequal merit. They deal with, amongst other matters, the use of tests, service crime, the psychiatric treatment of recidivism, narco-analysis and leucotomy.

Taking it all over, however, the Proceedings are worth glancing through if only because they indicate how much the scientific pursuit of delinquency is in need of co-ordination. They would have been much easier to digest and much more effective if they had been reduced by at least one-third and if the individual papers had been properly expanded.

E. G.

TOWARDS AN UNDERSTANDING OF JUVENILE DELINQUENCY. By Bernard Lander. (New York. Columbia University Press, and London, Oxford University Press, Geoffrey Cumberlege. 1954. Pp. xvi + 143. English price 24s. net).

This study of 8,464 cases of juvenile delinquency in Baltimore is one of the most important ecological-statistical investigations published in the field of criminology in recent years. With its mastery of statistical techniques and its well-balanced criticism of the pioneer work of Ernest Burgess, Clifford Shaw and their followers, it presents, if no final solution, at least another step forward 'towards an understanding of juvenile delinquency'. There is hardly anything new in the author's methods or findings: they are valuable mainly because they avoid the pitfalls resulting from the use of imperfect statistical techniques and from premature generalizations. The dangers of the 'all-too-frequent utilizations of the zero order correlation coefficient between delinquency and a specific variable' are well demonstrated (p. 70). Closely following the prescription of MacIver, who contributes the Foreword, the author stresses the need for slow and gradual progress from mere statistical correlation to causal explanation: 'methodologically, we have stressed in each stage of the study the very different conclusions we would have reached had we stopped earlier in our statistical analysis' (p. 77). The subject of the relationship between Prediction and Causation, briefly touched upon in MacIver's Foreword, is further developed in the author's concluding chapter in terms which might possibly be open to criticism. While it is true that in a prediction study each factor is examined in terms of 'how much it contributes to the variance of the dependent variable' and that in a causation study the primary interest is 'how the factors interact and are meaningfully related to differentials in the dependent variable', it can hardly be said that this second aspect is entirely foreign to the concept of the prediction study.

With his more refined statistical techniques Dr. Lander is able to show that the principal claims of the Burgess-Shaw school contain at best only half the truth. His material does *not* support their hypothesis that 'the spatial (geographical) location of an area, as such, in terms of the distance from the city centre is of significance in the prediction and understanding of the delinquency rate' (p. 74). Nor are bad housing or the invasion of industry as such positively related to delinquency if certain other variables are held constant; and the rate of Negro population in an area is positively correlated to it only where that rate remains below 50 per cent., whereas in areas of the greatest Negro concentration the delinquency rate is low (p. 64). Negro delinquency is therefore seen not as a function of race but as one of social tension which is highest where the two racial groups approach equality in numbers. In short, the zonal hypothesis oversimplifies the actual pattern of delinquency distribution and should be replaced by an explanation on the lines of the Durkheimian concept of *anomie*. In a way, this brings us back to our starting point, and the author, while rightly claiming to have at least eliminated a number of seemingly significant factors, finally admits that in a positive sense he has not yet been able to provide the broader sociological-psychological explanation of delinquency for which we are searching. Anomic and social disorganization, though perhaps basic, is not enough; 'a second, independently operating factor is socio-economic in character' (p. 88). The present reviewer may be forgiven if he cannot regard this as a final solution. However, to provide anything final was not the author's object in this comparatively limited study. And most probably there is no final solution anyway. H. M.

SOCIAL CASEWORK IN GREAT BRITAIN. Edited by Cherry Morris. (London: Faber & Faber Ltd. Pp. 231. 15s.)

A second edition of a technical book is an indication that it has found a respected place in the literature of the subject. 'Social Casework in Great Britain'—edited by Miss Cherry Morris with contributions from many well-known social workers—is almost the only British book on Social Casework, and it has been playing an important part in the training of social workers, though it is no substitute for a book on basic methods—a long-felt need in British casework.

There are few changes in the new edition, but those that have been made seem to be in two areas in particular: firstly, that of the administration of services which include casework and secondly, the problems of specialization.

All the contributors lay even greater stress on the importance of casework within the administrative structure of the social services, and also upon the essential combination of the personal and professional elements in casework methods. 'There is an urgent need for the development of casework proper. There is a still more urgent need for the application of casework principles, a sort of diluted casework, in other fields, particularly in social administration' (Cormack and McDougall). Voluntary organizations can no longer claim a monopoly of the personal services which they once initiated. Equally, however, the statutory services cannot assume that the administration of highly individual social services calls for the same methods of working as those which are considered suitable for less personal functions. The administrator is often more concerned with providing services *equitably* than he is with providing them *appropriately*.

Caseworkers have learned how to make constructive use of the legal and administrative structure in work with their clients, the limitations of which are, of course, often, but not always, only an extension of the legitimate restraints which society places on individual freedom. Professor Minn's contribution on the work of probation officers illustrates this very well. It is not only the clients who learn how to tolerate more easily the frustrations of life; conflict with the administration often provides a salutary lesson for the social worker.

The growing movement against specialization in social work points up another aspect of the influence of the administrative setting on casework. Miss Young-husband's contribution demonstrates how the present administrative structure can lead to overlapping and over-specialization, and how greater concentration on the generic or common aspects of casework methods can help to reduce this. We would be wise, however, to bear in mind that the forms of administration grew out of attempts to meet the needs of the population. Our knowledge of the nature of personal and social problems has developed piecemeal over the years as those needs became obvious. For a long time social workers were over-concerned with the situations or behaviour which initially brought their clients to them. It has taken time for widely different symptoms to be grouped together and linked with common causes which in themselves might be treatable.

A better understanding of the importance of developmental psychology and the dynamics of family life now makes it possible for caseworkers to develop methods of working which are applicable in all fields. This does not mean, however, that the difference between, for example, neurotic and delinquent behaviour as symptoms of underlying psychological conflicts is unimportant. The more we learn about causation the more we are likely to find that, whilst there can

be a large area of general agreement about methods of working, the age of our clients or the syndromes they present may well demand highly-specialized skills, though probably not along the present lines of specialization.

Any discussion of specialization always brings us back to the administrator, the quality of whose work should be judged by the extent to which he overcomes the problem of overlapping functions and breakdowns in communication by better co-ordination and co-operation.

In some of the contributions there is sometimes a wistful air of nostalgia for the days of voluntary social work, combined with fears for the future—'It may be less easy as the years go by not to become hardened and set in methods of helping, yet the best workers remain imaginative.' More knowledge and skill will be certainly of little value if social caseworkers become rigid and unimaginative, but must attempts at greater objectivity have this result? Sometimes our fears are based on a misunderstanding of what is meant by being 'scientific' and by professional discipline.

The re-issue of this book is a timely reminder that, whilst all may not be well in the administration and operation of the British social services, a tremendous amount has been achieved in the past fifteen years, and social workers have themselves laid much of the foundation of the common ground on which they are now beginning to stand.

R. C. WRIGHT.

PREVENTION OF CRUELTY TO CHILDREN. By Dr. Leslie George Housden, O.B.E., M.D. (London: Jonathan Cape. Pp. 400. 28s.)

Dr. Housden is to be congratulated on having produced a most valuable report on this subject, which is the responsibility and should be the concern of every member of the community. The term 'cruelty' is used throughout the book in the broad sense to include neglect and mental cruelty. He says in the Foreword: 'I go so far as to say that any child who is frequently unhappy is the victim of cruelty, and the condition is not confined to the poor or uneducated. It is found in all classes.'

The book is divided into three parts. The first outlines conditions in the past in relation to destitute and unfriended children, living conditions, sweated and child labour, children as the victims of drunkenness, child insurance and death clubs, and baby farming. This is grim reading, and the first reaction is to turn with a sigh of relief to Part 2, which deals with the present. Complacency is, however, soon shattered by the truly appalling list of cases which Dr. Housden cites as only a small sample of what is occurring in our midst.

He considers child cruelty at the present time under the headings of: Modern Living Conditions; The Hopeless Home, which includes a chapter by Dr. Robert W. L. Ward, who has been dealing for many years with some of the most neglected of the nation's children; The Present compared with the Past; and then goes on to consider special aspects of cruelty. He very rightly begins this chapter by discussing the irresponsible father. There has been far too much stress laid on the irresponsible mother, and the fact that her irresponsibility is often due to the inability to maintain an all but hopeless struggle to cope with a large family of young children and an unsatisfactory husband is often overlooked. Every aspect that he deals with is illustrated by actual cases, some of which are so terrible that it is almost impossible to believe that they exist.

Part 3 deals with the future. The first chapter is entitled 'The Cradle of Cruelty' and deals admirably with the subject under the headings of personal, social and material environment. Dr. Housden discusses education in the home by health visitors and residential training for mothers. The extension of formal education in home-making and parenthood is then dealt with, and from this he goes on to consider prevention and punishment and puts forward proposals for means of prevention. These include sixteen suggestions under four headings. The headings deal with: (1) the improvement of the home; (2) the education of potential parents; (3) provision of a healthier social environment for the young; (4) the need that parents should be held responsible for the welfare of their children.

He points out that the idea of prevention of cruelty is something new. Children were in many cases succoured in the past and cruelty stopped, but not prevented. It is no longer regarded as the work of a special society to deal with cruelty, but rather as the function of the combined work of many bodies, the emphasis being on education. This, Dr. Housden says, is a task to be shared by a wide variety of instructors, social workers, teachers and local authorities. It demands close and single-minded co-operation and a never-failing purpose to do what is best for the child. He points out what all those of us who are working in this field know only too well, that much good work for children is hindered by jealous unco-operation and that difficulties come less from multiplicity of direction, although this is undesirable, than from departmental parochialism. He gives the city of Exeter as an example of a well-run and satisfactory co-ordinating committee and says that if the whole country could follow this example no other sources of inspiration and endeavour would be needed. But if apathy and indifference exist in the general population and jealous departmentalism among those who are working in the field, there is little hope that anything of real value will be done to prevent cruelty to children in the foreseeable future.

Dr. Housden has dealt with the subject in a calm and temperate way. It lends itself only too easily to sentimentality and woolly thinking, both of which errors he has successfully avoided. As one who has much interest in this problem and some small knowledge of it, I regard this book as an invaluable contribution and I hope that it will be as widely read by all classes of the community as it deserves to be. Unfortunately, however, at the price of 28s. its circulation is likely to be limited. It would be of great benefit if it could be issued in a cheap edition that would be within the reach of those who are most concerned.

DORIS M. ODLUM.

PRACTICAL RESULTS AND FINANCIAL ASPECTS OF ADULT PROBATION IN SELECTED COUNTRIES. U.N. Department of Social Affairs, Division of Social Welfare. (New York. 1954. Pp. 112. 5s.)

In 1951, following the publication of 'Probation and Related Measures', the seventh session of the Social Commission of the United Nations recommended that there should be a further study and research on probation and related measures. Dr. Grünhut was asked to undertake this task in relation to adult probation, and his careful and painstaking work over three years has produced, from a study of an inchoate body of dissimilar and incomplete statistics and opinions collected from the orbits of twelve different legislations, a readable and stimulating report.

Dr. Grünhut reviews the use of probation for adults in the United Kingdom, the Scandinavian countries, the Netherlands and parts of the United States. He finds striking differences between one country and another in the frequency of the use of probation, a state of affairs which should, he suggests, stimulate fresh inquiries to determine whether there is room for extension into wider fields, especially among adolescent and elderly offenders, and also in relation to types of offences and offenders. He finds also that success rates in the countries in question are not susceptible of comparison because each country has experience of a different group of offenders and bases its assessment of success on a different standard. He concludes that 'adult Probation has come into its own and has been acknowledged, at least in principles, as a constructive method of treatment, indispensable, besides prison and other forms of institutional care, in any system aiming at individual prevention of criminality.'

The report contains an interesting chapter on predictability. Dr. Grünhut points out that, despite an impressive amount of research in the United States, work on prediction is still in its early stages. While the prediction methods available are, in his view, a reliable instrument for testing whether in a particular country there is room for an extension of adult probation, he stresses that they cannot inform the courts whether a particular individual will respond to probation. He pleads for further research into the subsequent conduct of probationers, and for the compilation of case studies which, though they must involve subjective judgment open to controversial interpretation, are essential for a proper assessment of probationers' attitudes and reactions.

Dr. Grünhut shows that the economic advantages of probation go much beyond the obvious fact that supervision in the open is less expensive than institutional treatment. He aptly concludes his report with a quotation from the 1949 report of the New York Department of Correction: 'Who can compute the value to society of offenders rehabilitated and families kept together?'

The desirability of promoting the use of probation for adults was unanimously accepted at the European Seminar on Probation, organized by the United Nations in London in 1952. Dr. Grünhut's report has become available at a time when many countries are contemplating the introduction of probation for adults, or an extension of its current use, and his study of practical results and financial implications should be of great value to them.

F. J. MACRAE.

THE CLINICAL INTERVIEW. Vol. II: A Method of Teaching Sector Psychotherapy. By Felix Deutsch and William F. Murphy. (New York: The International University Press Inc. 1955. \$7.50.)

The authors claim that this volume is a logical extension of Vol. I, which dealt with associative exploration. This volume is methodologically different in addition to having a directly therapeutic aim. The method is a goal, limited, and therefore deals with a 'sector' only of the patient's personality. In some respects it follows the method associated with the name of Finesinger and, although both procedures are offspring of Freudian analysis and the concepts are those of Freud, this offspring is not psycho-analysis. The authors merely recommend 'an approach which entails the active but unconscious interplay between physician and patient.' The authors state the important analytic aphorism that the patient does not know that his apparent conscious control of a situation is profoundly influenced by unconscious trends.

This is obvious to anyone who is in daily contact with the wanderings and windings of free association of patients, but this goal pursuit method by its very nature weights certain processes and accentuates doctrinal concepts and consequently cannot but encourage suggestion processes. Nevertheless, for teaching within the atmosphere of a hospital it is valuable, and under the guiding hand of the experienced analyst the student can be taught to be temperate in interpretation and moderate in his therapeutic optimism.

The interviews are given in detail with bracketed interpretative paragraphs which can only bring conviction to the already analytically initiated. The titles are romanticized—e.g., 'The Envious Man', 'The Weak Feet', 'The Overburdened Mouth'. There is a good psychosomatic study of hypertension, and one would have wished for a case of delinquency, for in this field the initial contacts are forced to be limited in method and in interpretation, and the doctor-patient nexus is of a very special quality which the word 'Transference' does not entirely cover until at later stages the procedure can become definitely analytic. E. M.

THE TECHNIQUE OF PSYCHO-ANALYSIS. By Edward Glover, M.D. (London: Baillière Tindall & Cox, Ltd. 1955. Pp. x + 404. 35s.)

This book was first published as a Monograph in 1928 by the International Journal of Psychoanalysis; based on lectures to trainees at the Institute, it constituted the only authoritative statement of the doctrine and methods of psychoanalysis at that time. In the last quarter of a century many developments and changes have occurred. The basic concepts have not only been further consolidated, but doctrinal differences have taken place both in England and America and psychoanalysis has been extended to cover the psychoses, the perversions, delinquencies and character deviations. It is because of this natural growth of a system and its methods of practical application that Dr. Glover felt justified in incorporating some of his later work and also to demonstrate the effects of the new expansion on methods of interpretation. The author holds primarily to the basic concepts of Freud which remain because, as he says, patients, neurosis and indeed analysis also do not radically change. He confesses that time has softened some early dogmatism and indeed he realizes that in conveying so intimate a technique as analysis and its subtle nuances of poured out material the communication of case material and its handling must be safeguarded against over emphasis of theory by condensation of the acquired data.

The layout of the book is an example of scrupulous scientific method applied to a subject and to dynamic material with which no other scientific discipline is confronted. All the main issues of the conduct of an analysis are fully described and their difficulties and ambiguities are discussed. The book of course will for a long time remain not only a guide to the student and to the perplexed but a starting point for many discussions not only by students but by experts. Written in a clear and at the same time lively style the book may make the novice feel that, having read the book, he can straight away practice what the author preaches. Students therefore must be warned that this is not a handbook but a treatise by one of our oldest practitioners in the field and also one of its most powerful controversionalists who recognizes that the science of mind analysis has still worlds to explore and perhaps never to conquer. E. M.

THE JUVENILE IN DELINQUENT SOCIETY. By Milton L. Barron. (New York : Alfred A. Knopf. Inc. 1954. Pp. xix + 256. \$5.)

This clearly written and fairly comprehensive text on juvenile delinquency has its frame of reference in the title where the idea is adumbrated that the problem has its locus in a delinquent society. Given such values as success, aggressive individualism, the prevalence of pecuniary standards, the alliance between politics and corruption, it is not surprising that juvenile delinquency which is but the expression of a criminogenic society should be widely diffused and recognized.

The first part of the book defines the problem and it is clearly shown that contemporary delinquency is not merely a pressing social problem but a unique concept. The dynamic aspect of delinquency is then considered, with special reference to the business cycle and war and peace. The second part is a survey analysis of the etiology of delinquency with special reference to the philosophy of causation, the biological and psychological aspects, the rôle of the family, peer groups and social institutions. The third and final part discusses society's reaction to the problem ranging from detection to detention to the setting up of the juvenile court and the various kinds of disposition and treatment that exist. A useful final chapter discusses delinquency control and prevention. JAY RUMNEY.

THE COMMUNITY AND THE DELINQUENT. By William Clement Kvaraceus. (New York : World Book Company. 1954. Pp. 556. Price not stated.)

Professor William Kvaraceus attempts in this book to show how children who are prone to delinquent behaviour can be spotted early in their lives by using all available scientific techniques. He shows how pre-delinquents and delinquents can be studied and how they can be given individual treatment using all the community resources for this therapeutic purpose. The contributions of the various parts of the community—the guidance clinic, the home and the school, recreation, the church, the police and the courts—are examined and tested and it is shown how a successful programme for reducing delinquency can be planned and carried through by the co-operation of professional workers, parents and the delinquent youngsters. The sub-title, 'Co-operative Approaches to Preventing and Controlling Delinquency', gives the essence of the book. In addition the author usefully provides a revised and enlarged Scale for Appraisal in Delinquency Prevention and Control and also check lists for evaluating the school's share in controlling the delinquency programme.

The most valuable part of the volume is undoubtedly the first five chapters where the author sees delinquent behaviour as a dynamic and continuous interaction of the child's personality with a troublesome array of external forces. His careful study of the differences between the delinquent and the non-delinquent—using his own earlier study and those of Healy and Bronner, Wattenberg's 'Boy Repeaters', Merrill's 'The Problems of Child Delinquency' and Sheldon and Eleanor Glueck's 'Unravelling Juvenile Delinquency'—constitutes a useful section of the book. It shows in great detail why and how some of the children 'have a bad time of it' when their lives are contrasted with the more orderly experiences of the non-delinquents.

This is a readable and stimulating book with a valuable coverage of the community's responsibility in so far as America is concerned and of the many ways in which the community may act in solving its own juvenile delinquency problem.

Most of the book was completed while the author was in Turkey working with the Ministry of Education. There was a definite advantage in seeing the whole problem from a distance and at the same time drawing upon the American scene. Some of the ideas in this book are mentioned in the author's well-known work, 'Juvenile Delinquency and the School', published in 1945. JAY RUMNEY.

THE WORKING-CLASS ANTI-SEMITES. A Psychological Study in a London Borough. By James H. Robb, M.A., Ph.D. Foreword by Prof. W. J. H. Sprott. (London: Tavistock Publications Ltd. 1955. Pp. 239. 15s.)

The phenomenon of Anti-Semitism—a very special and obstinate example of ethnic prejudice in general—has always attracted attention, not only because of its special importance but because its distribution arouses interest as a sociological and psychological problem which may throw light on the basic trends in society and in the individual. Dr. Robb's carefully planned study is therefore welcome, and that for several reasons. Firstly, because of his plan and method of inquiry; secondly, because of its attempt to relate regional life and individual behaviour; and, thirdly, because the specific problem itself needs ventilating at a scientific level.

The ecological study is particularly interesting in so far as he has chosen a very characteristic English area with a long and fairly stable history in which a not very thick population of Jewish origin has a trade and social connection for the better part of half a century. It would be a matter for discussion to decide whether his exploring of the character of individual members of this social group throws more than a focused light on the subject he has set himself to explore. Such characters are found at all levels of society and everywhere, and the tie-up with the ecology is not very clear. Nor is it clear whether the character traits of the anti-Semitic and the non-prejudiced carry a peculiar sign marking them off as anti-Semites otherwise in any special way.

Such character traits as Dr. Robb exposes will produce any sort of prejudice. It is in social history and other factors, religious and economic, that the pattern must be sought as well as in individual peculiarities. The Jew has become a standard figure of a scapegoat, and amongst members of a rather static and probably over-all suspicious group (as isolated groups tend to be) it would not be difficult for the insecure and the paranoid to seize on the most accessible victim for projecting self-hate and self-doubt.

Nevertheless this is a provocative study of method in social psychology, and from this point of view for the student of social and ecological factors in delinquency the inquiry will be rewarding as a guide to research even in fields of delinquent conduct. E. M.

BY COURTESY OF THE CRIMINAL. By C. A. Joyce. (London: George C. Harrap & Co. Ltd. 1955. Pp. 142. 8s. 6d.)

This is the story of the Cotswold Approved School, of which Mr. Joyce is the first and so far the only headmaster. May he long remain so. In the book, Mr. Joyce reminisces from time to time about his Borstal and prison experiences. To the scientific reader the book will at times be exasperating, for there are many generalizations, and occasionally great pleasure is taken in knocking down a chosen Aunt Sally even though she may have long been dead and buried. Mr. Joyce argues his belief in the use of corporal punishment in his school, but his chastise-

ment of those who do not agree with him seems to be directed against many who in fact do agree with him—that legal corporal punishment has no connection with the immediate administration of appropriate punishment on the spot by the person concerned. Nevertheless, the odd boy who wishes to be beaten or benefits from such treatment cannot be held to prove a theory. And there are some queer generalizations, such as: ‘I shall cease to believe in the deterrent effect of physical violence when I see little men hitting big ones.’ What on earth does this mean?

The book demonstrates the importance and value of personal contact with the offender and of maintaining institutions of sufficiently small population to make such personal contact possible. Someone visiting the Cotswold School said to Mr. Joyce: ‘But you are basing the whole thing on a matter of personal relationship’; and he replied: ‘Of course I am.’ Most institution workers would welcome the chance to do the same. The outlines here given of the Cotswold method, the day’s programme, the reception and treatment not only of boys but of all visitors, are all of great interest. Mr. Joyce may tend to simplify matters a little, but his belief in the therapeutic value of courtesy, absolute truth and fair dealings manifestly seen to be fair cannot be discounted. It will be a sad day if any modern scientific researches and theories lead to an effort to treat crime in the absence of these true values and of a full regard for the personality of each individual. This book carries a hint of a fear that this day may come. Mr. Joyce pleads for less psychiatry, but only on the ground that if psychiatrists were not so readily called upon they could do better work with fewer patients and their services could be employed where really needed.

It is a rambling and chatty book but gives an excellent picture both of the school and of C. A. Joyce.

F. DAWTRY.

ABSTRACTS AND REFERENCES

(Under the direction of W. R. BETT, with the co-operation of J. P. MARTIN, T. P. M. MORRIS and J. C. SPENCER.)

SOCIOLOGY

C. F. SCHMID and M. D. VAN ARSDOL, JR.: ‘Completed and Attempted Suicides: A comparative analysis’, *American Sociological Review*, 1955, **20**, 273–283.

This paper is a study of suicide behaviour in Seattle, Washington, during the period 1948–52, using the records of the Coroner and Police Department. Detailed data concerned age, sex, race, marital status, occupation, means and methods, and temporal and spatial distributions of completed and attempted suicides were subjected to statistical analysis. High rates were observed in the central business district and adjoining areas and relatively low rates in outlying residential sections. An attempt was made by the authors to isolate statistically an ‘efficiency’ component of the suicide complex’, as a basis for the development of a theory of suicidal behaviour.

J. C. S.

DANIEL GLASER: ‘The Efficacy of Alternative Approaches to Parole Prediction’, *American Sociological Review*, 1955, **20**, 283–287.

A controlled check is seldom made on parole predictions and the author shows the results of such a check on parole predictions made in the decade 1940–

49 at a fairly large penitentiary for inmates classifiable as 'young and improvable' in this paper. Some of the prognoses were made by psychiatrists and some by sociologists. The Mean Cost Rating represents the most suitable basis for comparing the selectivity of alternative predictors. The author is cautious about drawing conclusions from his data. He suggests that group factors are more relevant than personality factors to the prediction of parole outcome for most of the inmate population, and that the actuarial is superior to the case study method.

J. C. S.

W. J. H. SPROTT: 'Delinquent Worlds', *The Listener*, 1955, 53, 1013-14

Professor Sprott considers here the third of three approaches into delinquency-causation of which the statistical and case-history methods are the first two, namely the 'social background' research. His field material is drawn from Mr. J. B. Mays' 'Growing Up in the City' and his own research at Nottingham University, 'The Social Background of Delinquency'. The study of sub-cultural differences shows delinquency as part of a general climate of opinion prevailing in the society to which the delinquents belong. Thus it is, he argues, not only the parents and the emotional ties between them and their children that matter, but the whole set of standards and attitudes that are common currency in the household, the yard, and the street.

J. C. S.

RHODA J. MILLIKEN: 'The Police and Children in Trouble', *Federal Probation*, 1955, 19, 24-27.

Describes briefly the way policemen are confronted with numerous problems which they have to cope with on the spur of the moment, and in which they would be helped by having some social work training.

J. P. M.

HELEN L. WITMER and ELIZABETH HERZOG: 'And What About the Parents of Juvenile Delinquents?' *Federal Probation*, 1955, 19, 17-24.

This is a report of a conference held at the Children's Bureau in Washington in March 1954, attended by a group of experts drawn from various disciplines. The discussion included such topics as the value of Court action against parents—held to be worthwhile if used in carefully selected cases; group discussions for parents—whose value was both firmly upheld and vigorously questioned; and the individual approach through casework. It is interesting to note that it was alleged that 'in child guidance clinics the trend has been away from delinquents to middle-class and upper-middle-class children'. Representatives to the Chicago Area Project discussed the results and lessons drawn from the application of a broadly sociological approach to the problem, and it was agreed that such methods were complementary to more individual ones. Lastly some of the prevailing conditions that obstruct effective work with parents of delinquents were cited, of which perhaps the most interesting concerned the attitudes of the professions working in this field.

J. P. M.

SISTER ALOYSE SAXTON: 'Adoption', *Approved Schools Gazette*, 1955, 48, 385-386.

The writer questions the view that adoption and foster homes are necessarily preferable to orphanages or children's homes on the grounds that adoptive or foster parents are entirely unsuited to the task.

T. P. M. M.

- J. BOWLBY: 'The Rediscovery of the Family', Summary of 1st Sister Marie Hilda Lecture, *Approved Schools Gazette*, 1955, **48**, 400-403

The development of the Child Guidance movement is traced from 1930 to the present day. The importance of the family as a unit is stressed, together with the difficulties experienced by the child in making social relationships when it has been separated from its mother. T. P. M. M.

- R. H. ADAMS: 'Club Life and the Approved School Boy', 3rd Charles Russell Memorial Lecture, *Approved Schools Gazette*, 1955, **48**, 404-413

The rôle of Clubs and other Youth organizations in utilizing leisure time which might otherwise be spent in delinquent activity is discussed in respect of some 83 Approved School boys with Club experience. There is a danger of the club becoming an end in itself rather than a means to an end. The quality of youth work needs to be improved through better co-ordination, making use of such machinery as the Standing Conference of Voluntary Youth Organizations. Greater thought than in the past needs to be paid to the personal relationships of the individual club member. Both these courses are necessary to deal with the individuals who 'flit around' on the fringe of youth organizations. T. P. M. M.

- J. C. SPENCER: 'Delinquents and Their Neighbours', Notes of I.S.T.D. Lecture of 20.4.55, *Approved Schools Gazette*, 1955, **49**, 46.

The Director of the Bristol Social Project discusses the spatial distribution of delinquency in the city and concludes that certain areas of the city, of low social status, require special measures directed towards the development of family and community life. T. P. M. M.

SOCIAL THERAPY

- MARGARET TILLEY: 'Casework with the Anti-Social Client', *The British Journal of Psychiatric Social Work*, 1955, No. 11, 26-31

The writer, who is a psychiatric social worker at Holloway Prison, emphasizes the need to adapt the orthodox methods of work to meet the problem of the anti-social client. There is a temptation either to identify with society or with the client. She discusses the place of feeling on the part of the case-worker and the nature of her relationship with the client. The mixture of sympathy and scientific appraisal makes great demands on the worker. To an even greater extent than with other kinds of client, the assessment of the case involves a great deal beside the consideration of the actual symptoms. Perhaps the most useful work is often done on the nearly-conscious level. The question is discussed—should the worker take a moralistic line or be entirely tolerant and permissive? J. C. S.

- MARIE DUFFIN: 'Reaching Out to Prevent Delinquency', *Federal Probation*, 1955, **19**, 27-35

Describes, through the medium of one long case study, one aspect of the work of the New York City Youth Board. Aggressive casework has been developed to tackle cases that were members of problem families rather than delinquents as such. A follow-up study of 310 children referred in 1950 showed that three years later 41.5 per cent. had 'Improved'. The article, however, is concerned more with methods than an assessment of results. J. P. M.

AUSTIN H. MACCORMICK: 'The Potential Value of Probation', *Federal Probation*, 1955, **19**, 3-8

Interesting to the British reader because of its discussion of the place of Probation in American correctional work. Deals with the economics of Probation as opposed to institutional treatment, and stresses the enormous local variations in the extent and quality of probation services to be found even within one State.

J. P. M.

F. R. GEORGE: 'Are Approved Schools on the Way Out?' *Approved Schools Gazette*, 1955, **48**, 386-389.

The Approved School system has been undergoing a process of change since 1925, with modern theories emphasizing the need to treat the child and the home simultaneously, or to transplant him from one home to a better one. The institutional system of 'Houses' and Housemasters and Housemothers is essentially a compromise in this direction. Treatment of the child in an institution tends to ignore the roots he has put down in his own home, and may only in fact be useful for a minority of delinquents. It is noted that the recent closing of Approved Schools has been paralleled by a considerable increase in the provision of foster homes and facilities for helping families such as the F.S.U.

T. P. M. M.

F. R. GEORGE: 'Where To——?' *Approved Schools Gazette*, 1955, **49**, 48-49

The Approved School of the future will not receive criminals because it will have been realized that instead of *preventing* delinquency they have merely been treating delinquents. The schools of the future will be largely concerned with maladjusted children, taking them at the potential rather than at the actual delinquent stage.

T. P. M. M.

SEXOLOGY

W. RAEBURN: 'Sexual Diversion', *Approved Schools Gazette*, 1955, **49**, 11-14

No informed person denies that homosexual practices exist in Approved Schools, but they are largely *diversions* rather than *perversions*. They are merely one kind among several diversions of the normal sexual impulse which cannot be lawfully gratified until long after their awakening. It does not follow that the diversion must necessarily take a homosexual course. One school of thought advocates lenient measures to deal with the problem, based on the view that it is not thereby given the glamour of a secret vice and the boys will grow out of it. The other believes that this would sap the foundations of discipline to such an extent that a harsh regime such as the authorities were so anxious to avoid would be forced on them.

The former method seems most suitable for the smaller school, while the latter is probably necessary in the larger type of establishment.

T. P. M. M.

C. LAMBERT: 'Homosexuals', *Medical Press*, 1954, **232**, 523-6.

The ætiological factors and clinical types of homosexuality are discussed and represented diagrammatically. There is a stable, successful homosexual community which does not require psychological treatment. Psychoanalytic treatment is of limited value. Hormone therapy only modifies the intensity of sex desires. An endeavour should be made to find every unstable homosexual a suitable job which will give him emotional outlet and satisfaction.

W. R. B.

CORRESPONDENCE

TO THE EDITORIAL BOARD.

SIRS,

In the editorial notes of your issue of April 1955, you say at pp. 260–261, in reference to the problem of delinquency: ‘If only because of the sheer weight of numbers, comparatively little can be done for the majority of first offenders, and reformatory efforts have to concentrate mainly on the smaller, though still uncomfortably large, number of those who relapse again and again.’

I appreciate that this is a generalized statement, but in so far at least as it may be taken to be applicable to the position in this country, I think it right to suggest that it may give a misleading impression. In fact the Prison Commissioners have long acted on the view that it is perhaps their most important duty to ensure that an offender’s first experience of prison shall, if possible, be his last. And I hope I may show that their practice is in full conformity with that principle.

To begin with, all prisoners falling into the Star Class, which includes those coming to prison for the first time and others thought fit to associate with them, are from the outset segregated from others and (save in exceptional cases such as very short sentences) sent to prisons set apart for their class. These may be either central prisons for long sentences (over 3 years), regional prisons for shorter sentences which are yet not too short to permit of constructive training, or local prisons for the shortest sentences. And whatever the length of his sentence (unless it be only a few weeks) every Star prisoner who is suitable for treatment in open conditions will go to an open prison.

Certainly in the central and regional prisons, whether open or closed, these first offenders will receive the fullest and most constructive training that is at present available, in no way inferior to what can be provided for recidivists. Even for that large number whose sentences are too short to permit of this complete training, the effect of the open local prisons (such as that dealt with by Mr. Vickers on pp. 308–312 of the same issue) is bracing and valuable, and to use Mr. Vickers’ words ‘the whole function of the regime is educative’.

It is perhaps a secondary issue, but I would also suggest that at least in this country the ‘sheer weight of numbers’ does not appear to fall where you place it. From the table on p. 38 of the Annual Report of the Prison Commissioners for 1953 it would seem that of receptions into prison in recent years, only some 25 per cent. have been ‘with no previous proved offences’. And from para. 24 on p. 18 the ratio of the Star Class to the Ordinary Class in the daily average population seems to be of the order of 4:7.

Yours faithfully,

LIONEL FOX,

July 16th, 1955.

*Chairman of the Prison Commission
for England and Wales.*

Editorial Note: To some extent, the difference between Sir Lionel Fox and ourselves seems to be one of terminology. The sentence to which Sir Lionel objects appears in an editorial Note drawing attention to the forthcoming International Congress on 'Recidivism', and although the legal and penological definitions of this term show very wide variations it is unlikely that the work of the Congress will be predominantly concerned with those who are sent to prison for a second offence. Its primary concern will be those who, in the words of our Note, 'relapse again and again', and are sent to prison again and again. Although, as Sir Lionel admits, our Note was intended to draw attention to a universal problem rather than to the position in this country, it might be pertinent to say that the figure of 75 per cent. (p. 38 of the Prison Commissioners' Report for 1953) refers to men received in prison who had 'previous proved offences', which means not necessarily previous prison sentences or repeated prison sentences. The question of how far the 'first offender' is identical with the 'short term' prisoner cannot be reliably answered from published statistics, but it is likely that there are many more first offenders than recidivists among those 68 per cent. male prisoners who go to prison on sentences of not more than 6 months (p. 15 of the Report). In spite of the remarkable efforts made in recent years by the Prison Commissioners to increase accommodation in establishments of the open or medium security type (see p. 34 of their Report for 1953: 'there is now satisfactory camp accommodation for all short-term Star prisoners') it still remains unlikely that any lasting impression can be made on prisoners whose stay in these camps is of very short duration only. Mr. Vickers, to whose description of the excellent educational work at one of these open prisons Sir Lionel refers, himself draws attention to the difficulties arising from the 'shortness of the average length of stay' (*Brit. J. Delinq.*, Vol. V, No. 4, p. 310).

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